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EXPERIMENTS ON BINOCULAR VISION

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ABSTRACT. The paper describes the results of measurements of binocular acuity using constant and flash illumination, both with the unaided eye and a stereoscope. The effects on binocular acuity of varying the illumination, the time of exposure, and the field are considered and the relation between binocular acuity and form-sense is discussed.

MEANING OF BINOCULAR ACUITY

THE "visual acuity" is the minimum angle between two points which are resolvable. For points this limiting angle has a value of about 1 minute (Helmholtz (1), p. 256). It has a smaller value for specially selected objects such as two vertical lines separated by a horizontal line. Then a limiting angle of under 10 seconds between the vertical lines can be detected (see Hofmann (1), p. 55). This will be

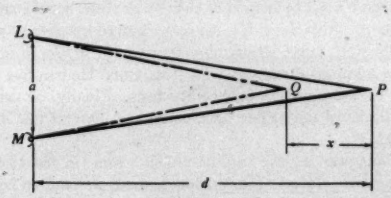

Fig. 1

called the "vernier acuity." The "binocular acuity" is the minimum resolvable difference in depth. It is measured by the smallest difference in binocular angle or parallax which can just be appreciated.

Let *a* mm. be the interocular distance of an observer who is converged on a

point P (Fig. 1) situated near his median plane and d mm. distant (d is large compared with a). The binocular angle, or parallax, LPM is

$$\frac{a}{d} \text{ radians or } \left(\frac{a}{d} \times 206000\right) \text{ seconds.}$$

Q is a point which lies x mm. on the observer's side of or beyond P. Q is nearly in the fixation plane LMP. The binocular parallax of Q is $\left(\frac{a}{d \pm x} \times 206000\right)$ seconds. The difference in binocular parallax, η, is given by

$$\eta = L\hat{P}M \sim L\hat{Q}M = \frac{x \times a \times 206000}{d^2} \text{ seconds, approximately.}$$

When it is just appreciated that Q is in front or behind P, the quantity η is a measure of the binocular acuity.

The Experimental Determination of the Binocular Acuity

Suppose that instead of one point P we have two points P_1 and P_2 (compare Helmholtz (1), p. 790) situated d mm. from the observer and in his horizontal plane. The join of these points is approximately normal to the median plane. A third object Q can be moved between the other two, towards or away from the observer. The observer's head is fixed so as to exclude monocular parallax. There are two experimental methods available for the determination of the binocular acuity.

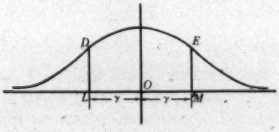

Fig. 2

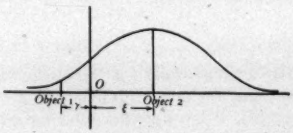

Fig. 3

(1) The observer may, by means of a screw or slide, set the middle object into the same plane as the other two. He can make a large number of settings and thus determine a probability curve whose abscissae are the errors in setting and whose ordinates are the number of settings with this error. He can test the adequacy of his data by one of the standard probability tests. Finally, he can determine the probable error (limits of the 50 per cent. zone) and convert this into difference of binocular parallax.

(2) An assistant may set the middle object γ mm. in front of or behind the plane of the other two objects. The setting is made at random by pulling a card out of a pack (see Howard (1)) which contains as many cards marked "in front" as "behind." Suppose A is the number of correct estimates of the position of the middle object, and B is the number of incorrect estimates. The ratio $\frac{A-B}{A+B}$ is calculated and the percentage $\frac{A-B}{A+B} \times 100$ obtained.

The value of η which corresponds to values of 75 per cent. or 50 per cent. can be defined to be the value of the "binocular threshold."

The relation between the two methods is an interesting one, and so far as I am aware it has not been explicitly stated hitherto.

Fig. 2, say, is the probability curve obtained from method (1). Then if A and B are the correct and incorrect estimates when the middle object is γ mm. in front of or behind the other objects P_1 and P_2,

$$\frac{A-B}{A+B}=\frac{\text{Area } DLME}{\text{Total Area of Probability Curve}},$$

very nearly, for the conditions under which these experiments are carried out. Hence, if $\frac{A-B}{A+B}=\frac{1}{2}$, L and M mark the limits of the 50 per cent. zone; if $\frac{A-B}{A+B}=\frac{3}{4}$, L and M mark the limits of the 75 per cent. zone.

The probability curve obtained by method (1) depends on the distance of the objects from the observer. The distance which we shall choose will be 6 metres because at this distance changes in accommodation play a negligible part in the estimation of fine differences of depth. γ is usually under 100 mm., more usually under 50 mm. Hence in this range (6000 $\pm$ 100 mm.) we may assume that the probability curve is constant. The equation of this curve is taken to be $y=be^{-h^2x^2}$. The probability that object 1 (Fig. 3) will be estimated to be in the same plane as object 2 depends on the ordinate of the probability curve at 1, i.e. on $be^{-h^2(\gamma+\xi)^2}$. If object 2 were at a distance $\xi+d\xi$ from O this ordinate would be $be^{-h^2(\gamma+\xi+d\xi)^2}$. Therefore the probability that object 1 is considered to be in the space beyond O is proportional to

$$\int_0^\infty e^{-h^2(\gamma+\xi)^2}d\xi;$$

i.e. B is proportional to

$$\int_\gamma^\infty e^{-h^2x^2}dx.$$

Similarly the probability that 1 lies on the same side of O as the observer is

$$\int_{-\infty}^{\gamma} e^{-h^2x^2}dx,$$

to which A is proportional.

Thus
$$\frac{A-B}{A+B}=\frac{\int_{-\infty}^{\gamma}e^{-h^2x^2}dx-\int_{\gamma}^{\infty}e^{-h^2x^2}dx}{\int_{-\infty}^{\gamma}e^{-h^2x^2}dx+\int_{\gamma}^{\infty}e^{-h^2x^2}dx}=\frac{\int_{-\gamma}^{+\gamma}e^{-h^2x^2}dx}{\int_{-\infty}^{+\infty}e^{-h^2x^2}dx}.$$

Historical Summary

Binocular Acuity

The first experiments to determine the value of the binocular acuity were carried out by Helmholtz (1), p. 790). He used three vertical needles, the centre one of which could be moved. The needles were 12 mm. apart and 340 mm. from the observer. He could "with perfect certainty" appreciate the middle needle being $\frac{1}{2}$ mm. out of the plane of the other two. He was *never* in error as to the position of the middle needle when it was $\frac{1}{4}$ mm. out of the plane of the extreme needles. The latter result gives $\eta = 30''38$ seconds, and the former $\eta = 60''5$ seconds. Helmholtz chose the latter value ("mit vollkommener Sicherheit zu erkennen"). This is carefully discussed by von Rohr (Czapski-Eppenstein(1), p. 400: see also von Rohr(1), p. 159). The value 60·5 seconds is much too high. Helmholtz was myopic and hence the short distance, 340 mm., from the observer to the object. This is one of the rare occasions on which the great scientist was prejudiced and reached an erroneous result. He wished to believe that the binocular and visual acuity were equal in numerical value. In case any further doubts arise as to this error the following should be conclusive. Stratton(1) also considered that the Helmholtz value of η was too large. Taking $\eta = 60''5$ seconds and the interocular distance as 68 mm. we obtain a value of 230 metres for the "radius of stereoscopic vision," i.e. for the distance beyond which there is no binocular perception of depth. Stratton(1) measured the radius directly by means of a mirror pseudoscope (see von Rohr(1), Abb. 110) so devised that the virtual position of the eyes was interchanged by the instrument but the interocular distance remained unaltered. Thus a flagstaff would appear to be in front of the sky using the unaided eyes but behind it using the pseudoscope. He moved the pseudoscope away from the flagstaff until these sensations disappeared and he found that the distance of the staff was then about 580 metres. This was the radius of stereoscopic vision and, his interocular distance being 65–66 mm., he derived an η value of about 24 seconds. Bourdon (1), p. 248) repeated Helmholtz's experiments. He used three needles at 2 m. and in one series of experiments the angular distance from the middle needle to the "wing" needles was 5 minutes, in another 1 degree. In both cases he was *certain* of the position of the middle needle when $\eta = 5$ seconds. When the angular distance is increased from 1 to 6 degrees Frübose and Jaensch (1), p. 126) found a very marked increase in the value of η.

Heine(1) repeated similar experiments at 5 m. and obtained $\eta = 5$–6 seconds. This value gave him an error-free series of observations. In another paper(2) he gives $\eta = 13''4$ seconds for an average observer at 5 m., while he himself obtained values of $5\frac{1}{2}$ seconds.

Hering (4), p. 22) states definitely that the accidental agreement of 60·5 seconds with the resolving power caused Helmholtz to jump at a conclusion which subsequent investigation would have shown to be erroneous. In consequence Helmholtz was unable to forestall the work of Hering.

Pulfrich (3) states that one can no longer hold the value of 1 minute obtained by Helmholtz. He says that in selected cases using the unaided eyes acuities of 10 seconds and less can be obtained. These results have been discussed from the physiological standpoint by Hering (4) and Heine. We shall reserve our discussion until later.

Crawley (1) found that out of 20 observers 18 had the certainty of correct estimation of depth when η was less than 20 seconds. One of these had the astonishing value of $\eta = 2{\cdot}3$ seconds.

Howard (1) uses essentially the apparatus of Brooksbank James (1). There are two rods, each 2 mm. in diameter, and they are mounted in a box so that the middle portion of the rods alone is visible, and is 6 m. from the observer. One rod is fixed and the other can be moved towards or away from the observer along a line 6 cm. from the fixed rod. The binocular threshold chosen by Howard is the value of η corresponding to the 75 per cent. zone, i.e. $\frac{A-B}{A+B} \times 100 = 75$. Of 106 cases examined 14 had values of η between 1·8 and 2·07 seconds, 30 had an average of 3·67 seconds, 15 had average acuity of 5·5 seconds, 23 of 7·3 seconds, and 24 were between 10·6 and 136·2 seconds.

Andersen and Weymouth (1) used three vertical threads at 2 m. distance. The middle one could be moved towards or away from the observer. The distance between the outer threads was in one case 8, in the other 3 cm. They obtained yet smaller results than Howard, but of the same order.

Frübose and Jaensch ((1), p. 123) point out from theoretical considerations that it is inaccurate to calculate the binocular acuity from the distance of one object behind or in front of the plane through the other objects and normal to the line of sight. One should measure the distance from the Hering-Hildebrand horopter. In the experiments subsequently to be described I have neglected this correction since its amount in reality varies from individual to individual (Helmholtz (1), p. 801) and it increases with fatigue (*ibid.*, p. 802). In a recent paper Tschermak (1) has shown that the form of the horopter, for vertical rods or threads, depends on the colour of these objects relative to the background. Tschermak and Kiribuchi have shown also that the form depends on the duration of the illumination (the shortest illumination used in these experiments was about 1/100 second). Fischer (1) has demonstrated that there is a marked asymmetry in the horopter curve deduced from his personal observations. In any case for the dimensions of the objects used in my experiments this correction is negligible.

Use of Test Plates and Diapositives to find the Binocular Acuity

A further method of finding the binocular acuity is to fuse the images produced by two diapositives. Such diapositives with engraved systems of lines were supplied by Dr Czapski to Hering ((4), pp. 22, 23). He obtained limiting values for η of 11 seconds. Hofmann, using a test plate of the same kind, obtained values between 11 and 12 seconds.

Pulfrich (4), using his test plate in the stereoscope, obtained values of about 15 seconds.

Binocular Vision using Illumination of Short Duration

In 1841 Dove (1) published the classical results that drawings viewed in a mirror stereoscope could be fused when they were illuminated by an electric flash, the duration of which had been shown by Wheatstone (*Phil. Trans.* 1834) to be less than $1/10^6$ second. He explained his experiments in greater detail in 1853 (2). He used a series of discharges and occasionally saw the three-dimensional effect "with perfect distinctness," but he also saw the double images, i.e. the two projections from which the three-dimensional relief arises. He was convinced that no ocular alternation or movements such as had been postulated by Brücke were possible. Volkmann ((2), p. 349) in 1846 also states that he was satisfied that the depth sensation was distinctly visible when the illuminating spark only lasted for ·000001 second. He considers that there can be no ocular movements since "the quickest movements would necessitate a time nearly 50,000 times as great." Donders (1) considers that, since Dove nowhere states that the relief was evident at the first discharge, changes of convergence were possible. Recklinghausen (1), modifying an experiment of Dove (2), forms the images of an electric discharge in the posterior and anterior reflecting surfaces of a bi-convex lens. One flash is sometimes sufficient to show the existence of the depth effect to a fresh and inexperienced observer. He also describes an experiment with two 1-inch tubes which are converged on a point 12 or 18 inches distant and have lids of oiled paper pasted over their ends with two pairs of holes in the lids. The holes are arranged to be in a horizontal line. These pairs of holes could be fused and could produce the stereoscopic effect when illuminated by electric flashes. Generally several discharges were necessary, but sometimes only one discharge was required. Aubert ((1), p. 315) and Helmholtz ((1), p. 891) improved Dove's experiment by making a fixation hole in each of the stereoscopic diagrams. In this way they were able to produce a proper convergence when the discharge occurred. This made stereoscopic fusion easier and the double images more difficult to observe. Hering (1) looked with both eyes through a tube at a vertical needle almost in the middle of the field while an assistant dropped spheres of varying sizes to the right or left of the needle and in front of or behind it. S. G. van der Meulen (1) improved on Hering's apparatus. He made the balls drop across a limited field. His fixation point was 57·5 cm. from the observer and the balls remained 1/20 to 1/40 second in the field of view. He was chiefly interested, however, in experiments on observers with imperfect vision. Donders used electrical discharges as objects. One was a continuous weak discharge from a coil and the other a single spark. He estimated whether this latter was nearer or farther away than the former, which was his fixation point.

Greeff (1) simplified van der Meulen's apparatus, while still meeting some of Donder's objections to the original apparatus of Hering. The time of visibility of the dropping ball was reduced to 1/50 second. Using prisms, he experimented

on depth perception by parallel and diverging visual directions. The depth perception was the same in both cases so long as the nascent double images could still combine in the latter case. He measured the binocular acuity up to 20 metres and found that positive results were obtainable so long as the depth difference was large enough relative to the distance. As a mean value he gave the ratio between these as 2 : 100.

Early and Preliminary Experiments by the Author using Howard's Apparatus and Pulfrich's Test Plates

In 1922 the author constructed an apparatus similar to Howard's and measured the binocular acuity of a number of naval officers who were specialising in gunnery and doing courses at the Royal Naval College, Greenwich. In all cases the visual acuity and muscular balance (as tested by the Maddox groove at 6 m.) were good. The experiments themselves were performed at 6 m., using rods 2 mm. in diameter. The illumination on the screen at the back of the box was about 4 foot-candles. The observer's head was fixed by making him bite a rigid horizontal soft piece of wood. The results are given in Table I.

The author also made a series of measurements using Pulfrich's test plates (Pulfrich (3) and (4)) and a stereoscope. These test plates are excellent for giving a rough differentiation of the observer's acuity, but the limited number of estimations makes an accurate determination of η impossible from the use of two plates only. It is found that observers who have good binocular acuity with the unaided eye sometimes obtain poor results with the stereoscope. The results given in Table I are not those of observers of long experience but of experiments of about 2½ hours' duration in each case.

Table I. *Experimental Results using Steady Illumination*

Observer	Howard's test Angular field of 1° Limits of 75 per cent. zone	Pulfrich's test plates (focal length of stereoscope = 6 inches)
	secs.	secs.
1	1·8	55 (about)
2	5·4	60 ,,
3	7·3	25 ,,
4	4·4	25 ,,
5	17·0	40 ,,
6	3·8	35 ,,
7	5·4	45 ,,
8	3·6	25 ,,
9	3·4	25 ,,
10	5·0	35 ,,

About this time it was becoming increasingly evident to the author that Dove's experiment was of critical importance. It was decided to try some experiments with Howard's apparatus using electric flashes. I started examining the positions of the rods under these short illuminations. I was led, however, to abandon the

apparatus because of the possibility of an error arising through the observer being able to estimate the position of the rods by the relative size of their retinal images. It is true that the difference in the angular size of two 2 mm. rods, one at 6 m. and the other 10 cm. farther away, is only about 1 second. Volkmann (1) had shown that in the comparison between two bands near each other which have widths of 0·5, 0·9 or 1·3 mm. and are at a distance of 20 cm., an angular difference of 12·4 seconds can be detected with certainty. Nevertheless in order to avoid a remote possibility of such an error I used in my subsequent experiments quite a different arrangement of objects. Frübose and Jaensch ((1), p. 130) consider that the length of the rods introduces an error into experiments where rods are used. When the rod is moved towards the observer a small shortening in the actual visible length must take place. The determination of the "front" position of the rod appears to be easier than that of the "back" position. These experimenters consider that the memory of the previous position of the rod is the factor. The author thinks that the comparison of the angular height and breadth of the movable rod with those of the fixed rod or rods (if a stop is used the angular heights of the rods are equal) is the possible cause of an erroneous judgment of position.

Experiments to find the Binocular Acuity using Constant Illumination

Two types of apparatus were constructed. In the first the illuminated field was about 1° and in the second about 8°. The first apparatus consisted of a box, if it might be so called, which had a front, back, and bottom with struts on the open sides to make it rigid. To enable the experiments to be repeated with exactitude I shall give its dimensions. It was 70 cm. high, 78 cm. wide, and 63 cm. long.

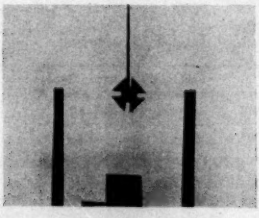

Fig. 4

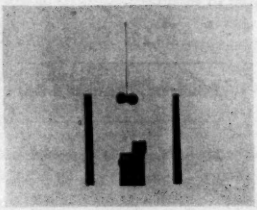

Fig. 5

There was a rectangular hole in the front which was 10 cm. high and 13 cm. long, and the centre of this hole was 25 cm. above the bottom of the box. In the back and exactly opposite the hole was a square aperture into which a plate of Ilford diffusing medium 22 cm. square was fixed. The object was a silhouette of a ship made of flat sheet brass with two masts and a funnel (see Figs. 4 and 5). The height of the masts was 6 cm., the width 3 mm., and the distance between their centres 6 cm. The funnel was midway between the masts and had a width of 1·75 cm.

and a height of 2·25 cm. The objects were of varying shapes and of about 1·5 cm. maximum width. They could be slipped into a ring on the end of a vertical strip whose thin edge was towards the observer. The objects were in line with the top of the masts. The top of the strip carrying the object was attached to a slider which could be moved towards or away from the observer. The position of the object with respect to the plane of the ship could be read on a millimetre scale which, of course, read zero when they were in the same plane. In some sets of experiments we used four different objects (say triangle, square, circle, and Maltese cross), in others six. The objects were replaced by new ones as soon as they had become familiar. A set of 24 cards was made, each of which gave the object to be used and whether it was to be put in front or behind. Each object was as often in front as behind

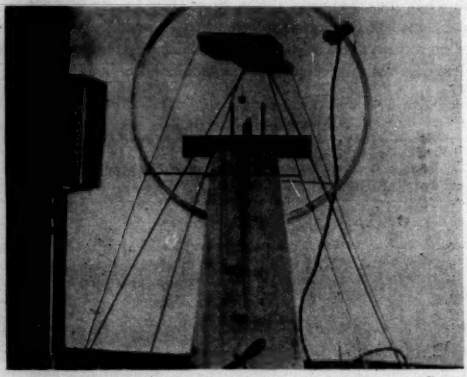

Fig. 6

and a card was drawn at random by the assistant who set the object. I think that for the purpose of determining binocular acuity a series of numbered circular discs of slightly differing diameters would be very satisfactory. The observer would never have any idea which was in use and could, from the size of the retinal images, form no opinions of their positions. However, we used objects of diverse shapes because we desired to compare the form sense with the binocular acuity under illuminations of short duration. There was a balanced trap door in front of the aperture which could be lowered when the assistant was setting the object. In the case of flash illumination we used, for direct observations, a small right-angle prism which was illuminated by a concealed faint lamp. This gave us a fixation point. It is seen on the top of the funnel in Fig. 5 and at the left-hand side in Fig. 4. For larger fields of about 8° the identical object carrier and slide from the first apparatus was mounted in the open structure of thin rods shown in Fig. 6. A large

screen of ground glass 71 cm. high by 91 cm. long was mounted behind the objects and this could be illuminated from behind, giving at 650 cm. an angular field of about 8°. This structure is shown in Fig. 6 where the small fixation prism is shown on the top of the funnel. The hoop and lamp, as will be explained later, were used for experiments with flashes when the objects were seen in indirect vision. In these experiments with the 8° field there was a curtain in front of the observer which could be pulled aside by him when the object had been set by his assistant.

Tests on the Observers

Four observers, A, B, C, and D, took part in the experiments immediately to be described. Their visual acuities at 6 m. were tested using an Orthops test chart; their muscle balance at 6 m. using a Maddox groove; their interocular distance (convergence on infinity) was measured by a Zeiss pupilometer. The correction to 6 m. in the last case was neglected. The particulars are given in Table II.

TABLE II

Observer	Visual acuity			Muscle balance		Inter-ocular distance
	Left eye	Right eye	Both eyes			
						mm.
A	6/6 (corrected)	6/6 (corrected)	6/6 (corrected)	1°·5 (exophoria)	0°·5 (right hyperphoria)	67
B	6/6	6/6	6/6	0°·25 (esophoria)	Nil	60
C	6/5	6/5	6/5	Nil	Nil	67
D	6/5	6/5	6/4	0°·5 (esophoria)	Nil	67

In the succeeding experiments the observer's head was fixed. The illumination on the screen was approximately uniform and was measured by a Trotter photometer. In Table III x mm. is the distance the object is in front of or behind the ship and η is the corresponding difference in binocular angle. The fourth column gives the number of correct (R) and incorrect (W) estimations of the position of the object. The fifth column gives the value of

$$P=\frac{A-B}{A+B}\times 100.$$

Experiment 1

Conditions.

Illumination	...	steady (produced by "Pointolite").
Intensity of illumination	...	4 foot-candles.
Field of illumination ...	...	1°.

TABLE III

Observer	x	η	Estimation of position		P
	mm.	secs.	R	W	per cent.
A	25	9·6	23	1	92
A	20	7·6	19	5	58
B	25	8·6	23	1	92
B	20	6·9	20	4	67
B	10	3·4	20	4	67
B	5	1·7	14	10	17
C	15	5·7	20	4	67
C	10	3·8	18	6	50
C	5	1·9	14	10	17
D	20	7·6	23	1	92
D	15	5·7	11	1	83
D	10	3·8	17	7	42

From these results one can conclude that the values of the binocular 50 per cent. threshold angle are very approximately as follows:

Observer	Value of binocular 50 per cent. threshold angle
	secs.
A	7
B	3
C	3·8
D	4

C and D had previously obtained the results given in Table IV, using Howard's apparatus.

TABLE IV

Observer	x	η	P
	mm.	secs.	per cent.
C	20	7·6	100
C	15	5·8	80
C	10	3·8	80
C	5	1·9	75
D	20	7·6	85
D	15	5·8	80
D	10	3·8	60
D	5	1·9	30

Twenty estimations were made in each case, using a steady illumination.

Experiment 2

The apparatus with the 8° field was used. The illumination was of the same strength as in Experiment 1, being nearly uniform and being produced by five suitably placed electric lamps. The results are given in Table V.

Conditions.

Illumination	steady.
Intensity of illumination	4 foot-candles.
Field of illumination	8°.

TABLE V

Observer	x	η	Estimation of position		P
	mm.	secs.	R	W	per cent.
A	20	7·6	20	4	67
A	15	5·8	18	6	50
B	10	3·4	23	1	92
B	7·5	2·6	19	5	58
B	5	1·7	27	13	35

Experiment 3

The same apparatus was used as in Experiment 2, but the screen illumination behind the objects was increased to 50 foot-candles, using a concentrated filament projector lamp. The results are given in Table VI.

Conditions.

Illumination	steady.
Intensity of illumination	50 foot-candles.
Field of illumination	8°.

TABLE VI

Observer	x	η	Estimation of position		P
	mm.	secs.	R	W	per cent.
A	20	7·6	17	7	42
B	10	3·4	21	3	75

Comparison of Results of Experiments 1, 2, *and* 3 *for Observers* A *and* B

1. Steady illumination of 4 foot-candles. Field 1°.
2. " " 4 " " 8°.
3. " " 50 " " 8°.

TABLE VII

Observer	η	P		
		1	2	3
	secs.	per cent.	per cent.	per cent.
A	7·6	58	67	42
B	3·4	67	92	75

It is difficult to conclude anything definite from these results. In A's case it appears that neither increase in field nor of illumination has made any considerable alteration in the binocular acuity. In B's case it might perhaps be inferred that increasing the field but not the illumination has somewhat improved the binocular acuity.

It is interesting to see whether the visual acuity is improved under similar conditions. Uhthoff (1) states that the visual acuity increases for illuminations ranging from ·5 to 3·5 foot-candles and is steady up to 10 foot-candles, but many people experience a falling off in the visual acuity for high illuminations.

The effect of an increase in the field on the visual acuity has been investigated by Wertheim (1), but his conditions are different from those experienced above. Using a grating to cover a wide area he states that the visual acuity decreases when the field is cut down by a stop. Frübose and Jaensch (1), p. 128) found that the length of the threads visible in their apparatus had probably some effect on the perception of depth and this experiment is therefore analogous to that of Wertheim. I can find no record that experiments have been made on the binocular acuity when the test object remains in the middle of the field and the field is enlarged, i.e. on the effect on the binocular acuity of illuminating the extra foveal portions of the retinae. This is obviously important in considering the effect of increasing the field of binocular telescopes.

Experiment 4. *On the Binocular Acuity when a Large Extra Foveal Area of the Retina is illuminated*

The 1° apparatus first described was set up at 6 m. and illuminated with an illumination of 4 foot-candles. The object was viewed through a hole in a large white cardboard screen which was placed 175 cm. from the observer. This could be illuminated uniformly with an illumination of ·35 foot-candle and the 1° illuminated field containing the object and the ship was viewed through a circular aperture in the screen which had an angular diameter of 3°. Thus the field illuminated by the cardboard screen extended from 3° to about 35° in angular diameter. Careful observations of the binocular acuity were made, those with the screen illuminated being intermixed with those with the screen unilluminated (Table VIII).

It was concluded therefore that under the conditions of the experiment there was no appreciable alteration in the binocular acuity when a considerable extra foveal area of the retina was illuminated.

TABLE VIII

Observer	Number of observations	η	P	
			Illuminated	Unilluminated
		secs.	per cent.	per cent.
A	48	5·7	50	50
B	48	2·6	50	58

Experiment 9. Binocular Acuity using as Objects Three Points of Light in a Dark Field and in the same Horizontal Plane

It was decided to make experiments of a totally different nature using three small illuminated areas surrounded by a dark field. Three small lamps were mounted inside cylindrical tubes (see Fig. 7). The illumination from each lamp

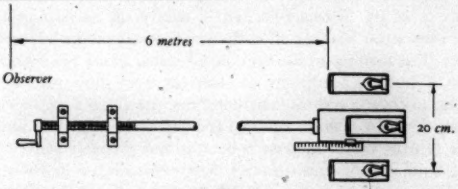

Fig. 7

fell on a ground glass screen fixed in the end of the tube and over this screen was fixed a disc with a small circular aperture. Thus in each case a small illuminated area was produced. Two of these tubes were fixed horizontally, 20 cm. apart, with their illuminated areas in a vertical plane normal to the line of sight and such that the join of these small areas was horizontal. The other tube was mounted on an optical bench so that it could slide in a horizontal plane midway between the other two tubes and towards or away from the observer. The movable tube could be moved by a long rod connected to a fine screw of 1 mm. pitch which could be operated by a handle at the observer's end. The observer was 6 m. from the objects. The lighted areas were placed in the same vertical plane and the illuminations carefully adjusted so that they appeared equally bright to the observer. By means of the screw observer A tried to set the middle light at the same distance as the other two. The position of the middle light was read off on a vernier and scale by an assistant. One hundred and fifty readings of the errors in setting were taken. They were analysed:

Greatest error towards observer $x = 57$ mm. or $\eta = 21{\cdot}8$ secs.
Greatest error away from observer $x = 54$ mm. or $\eta = 20{\cdot}7$ secs.
Mean error $x = 15{\cdot}2$ mm. or $\eta = 5{\cdot}8$ secs.
Probable error (binocular 50 per cent. threshold) $x = 13{\cdot}0$ mm. or $\eta = 5{\cdot}0$ secs.

Experiment 4 gave the binocular 50 per cent. threshold as 5·7 seconds. This last experiment is an important one and so far as I know has not previously been performed. The usual experiments with vertical threads, rods, etc. are of a very particular type and lead to theories like those of Hering and Heine which associate binocular acuity with the "vernier acuity" of Wülfing, and that appears to me to be correct only as far as it goes. Experiment 5, however, shows that we can have as fine a binocular acuity for points as we have for vertical lines. We shall discuss later whether the theory is sufficient that we can alter the fixation point until the two retinal images fall on disparate points even if η has such small values as 5 seconds or whether another theory can be advanced to explain this very fine binocular acuity for points of light.

Experiment 6. To find the Binocular Acuity using Three Points of Light in a Dark Field, the Extra Foveal Parts of the Retina being also illuminated

The same apparatus was used as in Experiment 5. A screen was erected 150 cm. from the observer and was faintly illuminated. The three light points were viewed through a circular hole in the screen 10 cm. in diameter. The angular diameter of this hole was thus about 4° and the extra foveal illumination extended up to an angular diameter of 30°. The middle light was, as before, set so as to appear at the same distance as the outside lights. One hundred and twenty observations were taken.

Greatest error in setting towards observer $x = 66$ mm. or $\eta = 25{\cdot}1$ secs.
Greatest error in setting away from observer $x = 68$ mm. or $\eta = 25{\cdot}8$ secs.
Mean error $x = 2{\cdot}13$ mm. or $\eta = 8{\cdot}2$ secs.
Probable error $x = 1{\cdot}8$ mm. or $\eta = 7{\cdot}0$ secs.
The binocular 50 per cent. threshold angle is about 7·0 seconds.

The effect of the extra foveal illumination seems to be to reduce slightly the binocular acuity. This was more evident than might be inferred from the numerical results. When the middle light was nearly in one of its extreme limiting positions and the screen light was extinguished it became much easier to estimate the correct position of the middle light. This was confirmed by several observers.

Experiments 5 and 6 were carried out in the Imperial College of Science and Technology, South Kensington, and the author must express his indebtedness to Professor F. J. Cheshire, C.B.E. and to Dr L. C. Martin for affording him facilities for carrying out the work and for their helpful interest.

Measurements of Binocular Acuity using Illuminations of Short Duration

A series (Experiments 7 to 20) of experiments was performed using the 1° and 8° apparatus, the illuminating sources being (*a*) an electrostatic discharge of very short duration and (*b*) the diminished image of the ball of a "Pointolite" lamp formed in the plane of an Anschutz three sector photographic shutter.

In case (*a*) a diagram of the apparatus is shown in Fig. 8. The electricity was generated by a Wimshurst machine connected to the inner coatings of two sets of Leyden jars, each set being in parallel. For simplicity a set is represented by one jar in the diagram. The inner coatings were also connected to a spark gap. A long cylinder of vulcanite on a quadrifilar suspension had a brass conical end-piece and could be pulled by a string so that the end-piece bridged the spark gap. The outer coatings of each set were connected to the illuminating gap which terminated in magnesium strips between two glass plates. This gap was mounted in the focus of a spherical mirror in the case of the 1° apparatus, so that the flash uniformly illuminated the diffusing screen behind the objects. In the case of the 8° field the illuminating gap was inside the focus of a 20-inch diameter searchlight mirror which was adjusted until the large ground glass screen (90 cm. by 120 cm.) was as nearly as possible illuminated uniformly by the discharge. The assistant would draw a card from the pack, set the object, extinguish his reading lamp and raise the trap door (or in the case of the 8° apparatus the observer would pull aside the curtain). Then the observer would fix on the faint illumination coming from the prism on the funnel and when the jars were charged the assistant would bridge the auxiliary gap with the pendulum and simultaneously discharge the illuminating gap.

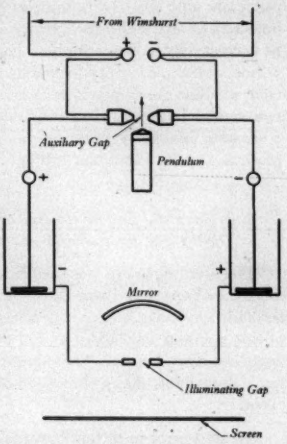

Fig. 8

To find the Duration of the Electric Flash used in the Experiments

There could be no doubt that the electric flash produced by the above method was of very short duration. Wheatstone (*Phil. Trans.* 1834 and see (3), p. 84) had examined the images of flashes in a hexagonal mirror rotating at a very high speed. The duration of the flash was less than $1/10^6$ second. Worthington, in connexion with his researches on splashes, had photographed fine radial marks on the edge of a rapidly spinning disc when these were illuminated by the electric discharge. He found no blurring of the marks in his photographs. I used the following method. I allowed the discharge to illuminate a sharp object (in this case a rectangular slit with two fine wires across its length). The image of this object was formed in a rotating mirror (silvered on the front face) driven at 1000 r.p.m. by an electric motor. Then the light passed through a photographic objective forming an image of the object on a photographic plate. The apparatus is shown in Fig. 9.

The problem was to discharge the spark at such a time that the mirror was in the exact position to form an image of the object on the photographic plate. The following arrangement was finally used. A thin disc with slots every 60° was mounted on the motor spindle. To its rim was attached an ebonite projection with a brass stud let through it. A spark gap was mounted on a fine micrometer screw by which it could be raised or lowered. The stud on the end of the ebonite projection passed through the gap every revolution of the mirror, and discharged the inner coatings in the same manner as the pendulum in Fig. 8. The camera had a very long extension and the plates used were quarter plates, speed 650. Behind the disc was placed an "Osglim" lamp on the 50 cycle A.C. supply and the disc could be kept at exactly 1000 r.p.m. in the dark without reference to the revolution counter by keeping the slots apparently stationary. The speed was controlled by a rheostat. The motor had its field and armature windings separated and the rheostat was used as a potential divider. The gap was adjusted by the micrometer screw until the image at 1000 r.p.m. fell in the middle of the plate. Measurement

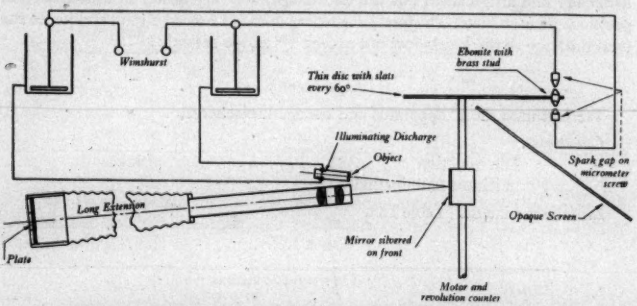

Fig. 9

Fig. 10

Fig. 11

and calculation showed that when the mirror was rotating at 1000 r.p.m. the image was travelling across the plate at about 285 metres per second. Hence a 1 mm. shift in the image would mean a total duration of illumination of 1/285,000 second. The photographs (actual scale) are shown in Figs. 10 and 11. Fig. 10 was taken with the mirror at rest and Fig. 11 with it revolving at 1000 r.p.m. The lens was an old portrait lens used at full aperture. The images suffer considerably from distortion and astigmatism but the two images are very nearly in exactly similar positions on each plate. The total broadening does not exceed 2 mm. and hence the total duration of the spark does not exceed 1/150,000 second.

Experiment 7

We first used the 1° apparatus and the spark discharge.

Conditions.

Illumination ... ... spark.
Field of illumination ... ... 1°.

Initial experiments (Table IX).

TABLE IX

Observer	x	η	Estimation of position		P
	mm.	secs.	R	W	per cent.
A	100	38·3	17	13	13
B	100	34·3	20	20	Nil
B	50	17·2	11	13	Nil
C	100	38·3	28	12	40
C	50	19·1	17	7	42
D	100	38·3	10	14	Nil
D	75	28·7	11	13	Nil
D	50	19·1	16	8	33

A and B had apparently a very poor binocular acuity. D had none for $\eta = 38{\cdot}3$ and 28·7 seconds but a 33 per cent. acuity for $\eta = 19{\cdot}1$ seconds. On the other hand, C had an acuity of about 40 per cent. for $\eta = 38{\cdot}3$ and 19·1 seconds.

Experiment 8

We will now give some results (Table X) obtained by A and B later when they had had a vast amount of practice in estimating depth perception using flashes and illuminations of short duration.

Conditions.

Illumination ... ... spark.
Field of illumination ... ... 1°.

It is quite apparent that with experience quite fine differences of depth can be appreciated when the illumination is that of the electric flash. The point of fixation was always the small prism.

It might be considered that alteration in the point of fixation increased the binocular acuity, but the increase shown above appears to be due to experience only.

It is interesting to note that Dobrowolsky and Gaine(1) found that the effect of experience on the peripheral visual acuity was to increase it to a maximum in 6 weeks, experimenting for 1 hour a day.

TABLE X

Observer	x	η	Estimation of position		P
			R	W	
	mm.	secs.			per cent.
A	100	38·3	37	13	48
A	50	19·0	35	15	40
A	40	15·3	17	7	42
B	100	34·3	42	8	68
B	50	17·2	34	16	36
B	40	13·7	19	21	Nil

In the above experiments the long positive after-image was violet in colour and persisted for about 10 seconds, but no details of the object were consciously discernible in it.

Experiment 9

The above experiments were repeated using the 8° apparatus (Table XI).

Conditions.

Illumination ... ... flash.
Field of illumination ... ... 8°.

TABLE XI

Observer	x	η	P	
			1° field	8° field
	mm.	secs.	per cent.	per cent.
A	100	38·3	48	75
A	50	19·1	40	58
A	40	15·3	42	58
B	100	34·3	68	75
B	50	17·2	36	67
B	40	13·7	Nil	33

The long positive after-image was very bright and, although the outline of the ship was quite visible, the details were indistinct. All observers considered that they made their judgment of position on the flash and not on the after-image.

There seems no doubt that an increase in the field has produced an increase in the binocular acuity. Personally, whenever I saw for the first time the wide field being illuminated by the flash I felt that it was easier to estimate the depth difference and when we went back and repeated observations on the 1° field I felt an increased sense of strain. No such feelings were experienced when the illumination was steady and we passed from a wide field to a narrow one or vice versa. It seems to

me that the effect is not due to physiological but to psychological causes, the wide field producing a more rapid stimulation of the psychological processes, and stirring them into action.

Binocular Acuity under Illuminations of Longer Duration

Experiments were made using an Anschutz shutter which gave times of 1/200, 1/100, 1/50, 1/25, 1/10, 1/5, 1/2, and 1 second. The shutter was a new one and we checked several exposures, obtaining 1/82 sec. for 1/100 sec., 1/42 sec. for 1/50 sec., and 1/24 sec. for 1/25 sec. Unless otherwise stated, the order of the experiments is the order in time in which they were performed. Those below follow on the early spark experiments. The observer also estimated the shape of the object and the results of this are given in the 5th column.

Experiment 10

Conditions.

Duration of illumination ... ... 1/200 sec.
Intensity of illumination ... ... 4 foot-candles.
Field of illumination ... ... 1°.

TABLE XII

Observer	x	η	Estimation of position		Estimation of form		Position P
	mm.	secs.	R	W	R	W	per cent.
A	100	38·3	15	9	9	15	25
A	50	19·1	11	13	10	14	Nil
B	100	34·3	25	5	12	18	67
B	50	17·2	16	8	9	15	33
C	50	19·1	16	8	4	20	33
D	50	19·1	12	12	5	19	Nil

Experiment 11

A and B repeated the experiments using an illumination of 96 foot-candles. The results are compared in Table XIII with those in Experiment 10.

Conditions.

Duration of illumination ... ... 1/200 sec.
Intensity of illumination ... ... 96 foot-candles.
Field of illumination ... ... 1°.

TABLE XIII

Observer	x	η	Estimation of position		Estimation of form		Position P	
							4 f.-c	96 f.-c.
	mm.	secs.	R	W	R	W	per cent.	per cent.
A	50	19·1	14	10	15	9	Nil	17
B	100	34·3	17	7	18	6	67	42
B	50	17·2	17	10	18	9	33	26

Under these conditions there appears to be no very appreciable alteration in the binocular acuity when the illumination is increased from 4 to 96 foot-candles.

Experiment 12

Conditions.

Duration of illumination ... ... 1/100 sec.
Intensity of illumination ... ... 4 foot-candles.
Field of illumination ... ... 1°.

TABLE XIV

Observer	x	η	Estimation of position		Estimation of form		Position P
	mm.	secs.	R	W	R	W	per cent.
A	100	38·3	19	5	9	15	58
A	50	19·1	13	11	11	13	8
B	100	34·3	19	5	12	12	58
B	50	17·2	11	13	15	9	Nil
C	50	19·1	15	9	12	12	25
D	50	19·1	12	12	7	17	Nil

Experiment 13

Conditions.

Duration of illumination ... ... 1/50 sec.
Intensity of illumination ... ... 4 foot-candles.
Field of illumination ... ... 1°.

TABLE XV

Observer	x	η	Estimation of position		Estimation of form		Position P
	mm.	secs.	R	W	R	W	per cent.
A	100	38·3	21	9	11	19	40
B	100	33·4	18	6	14	10	50
At the end of several weeks B obtained the following results:							
B	50	17·0	22	2	20	4	83

It is interesting to note that the form sense has also markedly improved through experience.

Experiment 14

Conditions.

Duration of illumination ... ... 1/25 sec.
Intensity of illumination ... ... 4 foot-candles.
Field of illumination ... ... 1°.

TABLE XVI

Observer	x	η	Estimation of position		Estimation of form		Position P
	mm.	secs.	R	W	R	W	per cent.
A	100	38·3	20	4	13	11	67
B	100	34·3	17	7	16	8	42

Experiment 15

Conditions.

Duration of illumination ... ... 1/10 sec.
Intensity of illumination ... ... 4 foot-candles.
Field of illumination ... ... 1°.

TABLE XVII

Observer	x	η	Estimation of position		Estimation of form		Position P
	mm.	secs.	R	W	R	W	per cent.
A	100	38·3	21	3	11	13	75
A	50	19·1	29	1	23	7	93
A	42·5	16·3	22	2	15	9	83
A	35	13·4	17	7	20	4	42
B	100	34·3	27	3	23	7	80
B	50	17·2	27	3	27	3	80
B	40	13·7	19	5	20	4	58
B	30	10·3	16	8	22	2	33

Experiment 16

Conditions.

Duration of illumination ... ... 1/10 sec.
Intensity of illumination ... ... various.
Field of illumination ... ... 1°.

Observer C performed some experiments in which the illumination was varied (*a*) by moving source of light away from screen, (*b*) by interposing a rotating sector.

TABLE XVIII

Observer	x	η	Estimation of position		P	Remarks on illumination
	mm.	secs.	R	W	per cent.	
C	100	38·3	20	4	67	4 f.-c.
C	100	38·3	17	7	42	Cut down to 1 f.-c. by rotating sector
C	100	38·3	19	5	58	Cut down to 1 f.-c. by moving the light source
C	100	38·3	17	7	42	Cut down to 0·5 f.-c. by rotating sector

It was concluded that in the above case the decrease in the illumination (whether produced by rotating sector or otherwise) did not affect the binocular acuity to any great extent.

Experiment 17

Conditions.

Duration of illumination ... ... 1/5 sec.
Intensity of illumination ... ... 3·1 foot-candles.
Field of illumination ... ... 1°.

TABLE XIX

Observer	x	η	Estimation of position		Estimation of form		Position P
	mm.	secs.	R	W	R	W	per cent.
A	100	38·3	23	1	12	12	92
A	50	19·1	22	2	18	6	83
A	35	13·4	22	2	20	4	83
A	30	11·4	16	8	19	5	33
B	100	34·3	24	0	20	4	100
B	50	17·2	24	0	17	7	100
B	35	12·0	18	6	20	4	50
B	30	10·3	20	4	21	3	67
B	25	8·5	19	5	21	3	58

Experiment 18

Conditions.

Duration of illumination ... ... 1/5 sec.
Intensity of illumination ... ... ·46 foot-candle.
Field of illumination ... ... 1°.

TABLE XX

Observer	x	η	Estimation of position		Estimation of form		Position P
	mm.	secs.	R	W	R	W	per cent.
A	50	19·1	23	1	18	6	92
B	75	25·7	12	0	10	2	100
B	50	17·2	22	2	19	5	83
B	40	13·7	11	1	10	2	83
B	35	12·0	16	8	21	3	33

It would appear that B's binocular acuity is better for an illumination of 3·1 foot-candles than for that of ·46 foot-candle.

The Relations between the Binocular Acuity and the Form Sense

From an examination of the results given above it is concluded that we can sometimes have a quite high binocular acuity accompanied by a very poor sense of form.

It is true that both the binocular acuity and the form sense improve with practice, but one is inclined to believe that the binocular acuity must belong to a lower cortex than the form sense. Schoute (1) states that for persons of low visual acuity the binocular acuity is always significantly better than the acuity would

lead us to expect. Goldstein and Gelb (1) reached some interesting results in the case of a patient with an injured brain. He had a correct perception (see Hofmann (1), p. 93) of the individual parts of contours both as regards direction and as regards distance. What was lacking was the ability to combine the optical perceptions into a whole. The patient had a good central visual acuity but no form sense. Parsons (1) states that the acuity means the minimum separable, but the form sense means the minimum legible and cognisable.

TABLE XXI

Observer	Duration of illumination	Field of illumination	η	Estimation of position		Estimation of form	
	sec.	degrees	secs.	R	W	R	W
A	Flash	8	38·3	21	3	3	21
B	Flash	8	34·3	21	3	6	18
C	1/200	1	19·1	16	8	4	20
A	1/100	1	38·3	19	5	9	15
A	1/5	1	38·3	23	1	12	12

Considering these opinions and the above results it would appear that the form sense resides in a higher association centre than either the visual or binocular acuity.

A consideration of Experiments 7 to 18 leads us to the following approximate conclusions. For observers without much experience and for illuminations of and under 1/25 second the binocular acuity is relatively fairly poor compared with steady illuminations, the binocular 50 per cent. threshold angle being roughly about 30 seconds for observers A and B. For an illumination of 1/10 second it is about 15 seconds and for 1/5 second it is somewhat smaller. With experience higher acuities can be obtained.

Binocular Acuity in Indirect Vision using the Electric Flash

It was now decided to conduct some experiments to see whether the binocular acuity (for small values of η such as 30 seconds) extended beyond the centre of the fovea. Instead of having the fixation point near the centre of the object, we mounted a faint fixation light at a certain angular distance from the centre. When the flash illuminated the object, the observer was converged on this light.

Experiment 19

We used the apparatus with the 1° field, drilling three holes in the box at positions 1, 2, and 3 and mounting three lamps illuminating small apertures in the plane of the ship (see Fig. 12). These luminous points were in each case 2° from the centre of the silhouette. The lamps were controlled by three switches which were used at random by the assistant. Flash illumination was used. The relative position of the fixation light and the silhouette were as far as possible unknown to the observer. (Of course the ideal arrangement would have been to have the silhouette capable of rotation about the fixation light.)

It is evident that the visual acuity 2° from the centre of the fovea is relatively poor.

These experiments were repeated for flashes of 1/5 and 1/2 sec. For times of 1/5 sec. it was difficult to get results showing a high binocular acuity, but for 1/2 sec. the binocular acuity was very good. Presumably in 1/2 sec. the eyes can alter the point of convergence well towards the centre of the object. From the figures obtained by Erdmann and Dodge (Bourdon (1), p. 37) and from the results in Table XXII it appears that the movement of the point of fixation must be very much slower than that of one eye.

TABLE XXII

Observer	η	Position of fixation point and estimation of position					
		1		2		3	
	secs.	R	W	R	W	R	W
A	38·3	12	12	15	9	12	12
B	34·3	25	17	13	11	26	21

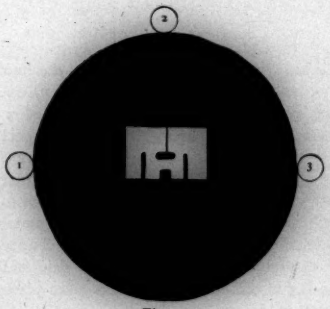

Fig. 12

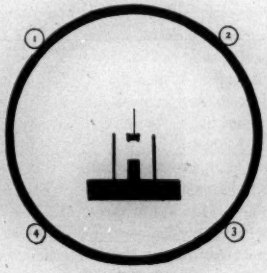

Fig. 13

Experiment 20

These experiments were repeated using the 8° apparatus. A hoop was erected (see Fig. 6) whose centre was the centre of the silhouette and whose radius subtended an angle of 2° at the observer. On the hoop was mounted a fixation lamp which was placed in the positions 1, 2, 3, and 4 shown in Fig. 13. In the results (Table XXIII), in addition to R and W, we introduce the interrogative Q which means that the observer cannot express an opinion as to the position of the object. Flash illumination was used exactly as in Experiment 9.

It is quite apparent that such fine binocular acuities do not exist 2° from the centre of the fovea. The experiments were not carried out long enough to see whether the binocular acuity might be improved by experience.

The visual acuity 2° from the centre of the fovea, according to Wertheim (1), is about one-half the acuity at the centre.

Von Kries (1) considers that the binocular acuity varies over the retina in a similar way to the visual acuity. Marx (1) agrees. Von Kries says further that the binocular acuity is quite large even at a considerable distance from the centre. Some interesting results for very indirect vision have recently been obtained by Fischer (2). By placing a small mirror in front of one eye, he was able to test the correspondence of elements of the nasal side of one retina with those of the temporal side of the other, which pairs of elements owing to the interposition of the nose cannot normally function together. Using the horopter method he found a binocular correspondence between these pairs of elements.

TABLE XXIII

Observer	η	Positions of fixation lamp and corresponding estimations of position											
		1			2			3			4		
	secs.	Q	R	W	Q	R	W	Q	R	W	Q	R	W
A	38·3	23	1	—	24	—	—	22	1	1	20	1	3
B	34·3	9	1	2	6	4	2	3	4	5	2	5	5

I was interested to see in the case of Experiment 20 whether any of the four positions of the fixation light gave more promise of positive results than the others. Observer A thought position 4 was the more promising, while B preferred 3. The matter is inconclusive and requires further experiment. We should expect to find the binocular acuity most highly developed in the lower part of the apparent field considering that man holds his head erect and that the indirect binocular acuity in the upper half of the apparent binocular field is of small importance to him in his daily life as a peripatetic animal compared with that below the horizontal plane. This would agree with Mandelstamm's (1) results. He found that the identity between corresponding points was least accurate in the upper apparent field and most accurate in the lower half, i.e. the fusion of the double images takes place least easily in the lower half of the apparent field.

Experiments on Binocular Acuity using the Stereoscope

I had an apparatus constructed as in Fig. 14. It was a modification of Pulfrich's stereo-micrometer and consisted of a flat brass rectangular frame of about the same outer dimensions as a stereoscopic slide. To the lower bar were attached four needles. Experiments showed that these needles were not distinct enough when illuminated by a flash and four very accurately made discs (not shown in Fig. 14) each of 3 mm. diameter were attached to the ends of the needles. The needles were adjusted so that the four discs viewed in the stereoscope fused into two, both of which appeared to be at the same distance from the observer. A fifth needle and disc were carried from the left half of the upper bar of the frame. A sixth needle and disc were carried on a slide attached to the right side of the upper bar. The

slide was attached to an accurate micrometer screw. In the stereoscope the two discs carried from the upper bar fused into a disc which could be brought in front of or behind the plane of the two fixed discs by turning the micrometer head.

Of course an angular displacement of the middle disc relative to the side discs occurs in the right field when the micrometer screw is moved. This is the disadvantage of the stereo-micrometer compared with test plates with fixed scales, but a very large number of the latter are required if numerical results are desired. In the experiments to be described the movements of the micrometer head are very small and observer A's left eye is his stereoscopic master eye. In consequence the shift of the middle disc in the right field is of no very great importance in producing a means of estimating the position in depth of the middle disc relative to the others. A sheet of ground glass was placed in the stereoscope behind the apparatus. The middle discs were coated with a phosphorescent substance to give a point of fixation.

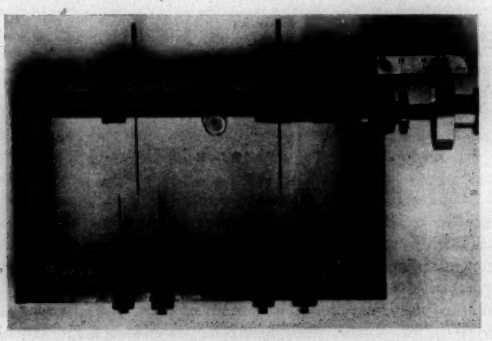

Fig. 14

Experiment 21. *To determine the Binocular* 50 *per cent. Threshold Angle using the Stereoscope and Steady Illumination*

One hundred settings of the micrometer were made so that the middle disc appeared to be in the same plane as the outer discs. The results are analysed below:

Mean of 100 settings of micrometer	10·140 mm.
Settings most in error from mean	10·185 and 10·095 mm.
Mean error of 100 settings ...	·018 mm.
Probable error	·0153 mm.

Taking the focal length of the oculars of the stereoscope to be 18 cm., we would find 17·5 seconds for the binocular 50 per cent. threshold for observer A. Pulfrich (4), using test plates, obtains 15 seconds, apparently for an error-free series of observations. Best (1), using similar apparatus, obtained regularly values from 20 to

40 seconds. The apparatus as above constructed does not appear capable of such high precision as test plates, and the definition of the stereoscope used was none too good: however, the above results are useful for purposes of comparison.

We can now construct a table which gives the binocular parallax differences between the centre disc and those on either side of it corresponding to the micrometer settings.

Experiment 22

The stereo-micrometer was placed in the stereoscope. Both sides were illuminated at the same instant by an electrical flash. The micrometer was set so that the middle disc was in front of or behind the other two discs by a known difference in binocular parallax. The setting "in front of" or "behind" was made by drawing a card from a pack as in the experiments on unaided binocular acuity and the observer estimated whether the middle disc was in front of or behind the other two.

TABLE XXIV

Observer	Date	η (difference in binocular parallax)	Estimation of position		P
		secs.	R	W	per cent.
A	1st day	180	24	0	100
A	,,	120	16	8	33
A	2nd day	120	23	1	92
A	,,	90	20	4	67
A	3rd day	90	22	2	83
A	,,	60	17	7	42
A	,,	45	15	9	25

This appears to show that experience very markedly improves the binocular acuity, and I have no doubt that with improved apparatus and greater practice finer binocular acuities could be reached. Pulfrich (5, p. 20) quotes an earlier statement and says that with the stereoscopic rangefinder it is easy "to find the range of objects visible only for short periods of time, such as birds in flight and splashes from the fall of shot, which have disappeared long before one has formed a true conception of their shape and form." The author is, however, not aware that experiments have been performed with the stereoscopic rangefinder using illuminations of such short duration as the electric discharge.

Experiments on Persistence in Stereoscopic Vision

Already we have raised the interesting question as to whether binocular acuity can be produced by the long positive after-image. It was stated that in experiments using the electric flash and with the unaided eye observers felt that they made their judgment on the flash illumination and that they were not conscious of any assistance being afforded by the after-image. Wheatstone (1, p. 266, or 3, p. 257) viewed in the stereoscope diagrams consisting of broad coloured red lines on a green ground. They were strongly illuminated and when the eyes were shielded from the light and the after-images had time to appear, and these images in their alternations were visible to both eyes simultaneously, the binocular image was seen in strong relief.

Rogers(1) obtained this effect by producing the two images not simultaneously but in succession, first the after-image of one diagram in one eye and then that of the other diagram in the other eye. The effect of relief was thus produced when both the after-images were simultaneously visible.

Helmholtz ((1), p. 891) had a true stereoscopic effect from positive after-images which he had developed through a momentary bright illumination of short duration. He points out that it is difficult to perform successful experiments with after-images. They must be very sharply defined. There is always a tendency, too, to project the images on to any real background which happens to be present and so to associate the details of the objects with this background.

It therefore appears that while under very favourable conditions stereoscopic relief can be produced in the binocular after-image, yet in such experiments as 7, 8, and 9 the binocular acuity is determined by the illumination of the object by the flash and not by the after-image.

Experiment 23

Apart from the possibility of fusing the after-images and so developing a perception of depth, it was always possible that the mind might retain some impression of the image presented to one eye and that a comparison of that impression might be made with the image afterwards presented to the other eye. The stereo-micrometer was mounted in a stereoscope and an arrangement (shown in Fig. 15) was devised whereby one side of the stereoscope could be illuminated by one flash and the other by another flash, the interval between the two flashes being adjustable and only two flashes being produced by the apparatus.

An ebonite disc was mounted on the spindle of a motor fitted with a revolution counter. This disc had a brass stud let through it some distance from the rim and as the motor rotated the stud passed through two spark gaps 180° apart. Each of these gaps was in series with an illuminating gap with magnesium end pieces. These magnesium gaps were built into cardboard tubes (which had to be soaked in paraffin wax) and one illuminated the right and the other the left side of the stereoscope. The outer coatings of the left pair of Leyden jars were insulated and all the wires were carried on insulating blocks of paraffin wax. An earthed metal rod, attached to a string which passed through a pulley, could be lowered on to the two left conductors. Then the outer coatings of the two jars on the left would also be earthed. The motor was started and the jars were charged up by the Wimshurst machine. When the observer at the stereoscope was ready, the assistant raised the earthing rod and pulled the discharging pendulum into the auxiliary gap which connected the inner coatings of the jars. Then the electrostatic charge on the outside of the jars discharged itself in succession through the illuminating gaps. The stereo-micrometer was set so that the difference in binocular parallax between the centre and the side discs was 180 seconds. The motor was separately excited and controlled by a rheostat beside the stereoscope. The interval between the flashes was shortened until the three objects stood out in stereoscopic relief, the position of the middle object in front of or behind the other two being apparent. The results are given in Table XXV.

TABLE XXV

Observer	Revolutions per minute	Intervals between the flashes which just produced stereoscopic vision
		sec.
A	1160	·0259
A	1220	·0246
A	1200	·0250
B	1040	·0288
G	1050	·0286
E	1000	·0300

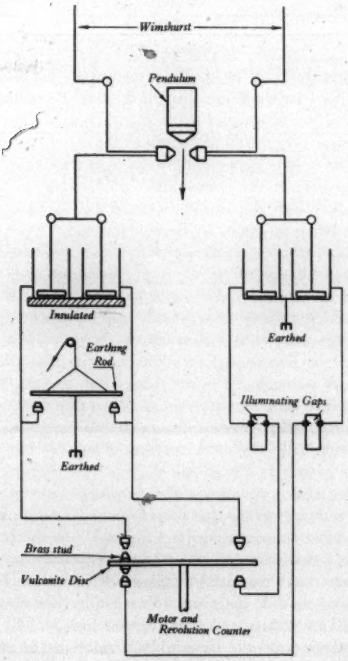

Fig. 15

The illumination was not very bright and the long positive after-image not evident. The mean interval between the flashes which just gave stereoscopic vision was about 1/36 second. This is not widely different from, but perhaps a little smaller than, the average duration of the persistence of vision.

Experiment 24

The last experiment was repeated, but each side of the stereoscope was illuminated in succession by a series of flashes of duration of about 1/500 second. This was produced by a fine slit in a disc which was carried on the spindle of the motor described in Experiment 23. Two "Pointolite" lamps were mounted at opposite sides of the disc and as the disc rotated the slit allowed the light from each "Pointolite" in turn to pass through a system of prisms and lenses, an image of the slit being formed in the plane of the stereo-micrometer by each system. As the disc rotated, the light from one "Pointolite" illuminated one side while that from the other "Pointolite" illuminated the other side of the stereoscope. The image of the slit could be considered to travel like a horizontal band of light vertically and successively across each side of the stereoscope. The middle disc in the stereo-micrometer was set so that the difference in binocular parallax with respect to the fixed discs was 180 seconds. The speed of the motor was adjusted until stereoscopic vision just appeared or disappeared. The results are given in Table XXVI.

TABLE XXVI

Observer	Revolutions per minute	Interval between successive illuminations
		sec.
A	240	·125
A	225	·133
G	170	·176

The mean of these is about 1/7 second. Hence the interval for successive repeated illumination of the left and right fields is about five times that for two successive isolated flashes. The explanation of this probably lies in the psychological effect of repetition. An infra-minimal stimulus may become a minimal stimulus when repeated sufficiently often at definite intervals.

Experiment 25

In the last two experiments the eyes were illuminated in succession, only one eye receiving light at a time. In the experiment now to be described, both eyes receive light simultaneously but two stereoscopic slides are used and they are presented in succession.

The stereo-micrometer had its four lower needles removed (Fig. 16 *a*) and was mounted in the usual slide carrier of the stereoscope (Fig. 16 *c*). A photographic stereoscopic diapositive (Fig. 16 *b*) was made and mounted horizontally on the top of the stereoscope (see Fig. 16 *c*). A metal frame was inserted in the stereoscope

and this carried two glass plates. These were silvered on the side next the oculars with a very fine semi-transparent film so that when the ground glass screens behind the diapositive and stereo-micrometer were equally illuminated the reflected image of the former and the transmitted image of the latter were nearly equally bright to the observer in the stereoscope. Using the rotating disc with the slit and the "Pointolites" as in Experiment 24, it was possible to illuminate the diapositive and the stereo-micrometer in succession. The duration of the illumination was again about 1/500 second and the intensities were nearly equal to those used in the last experiment. The micrometer was adjusted so that the fused disc seen by direct transmission was out of the plane formed by the two discs of the diapositive. When the motor was started first the stereoscopic image due to Fig. 16 *a* and then that due to Fig. 16 *b* was presented. The speed of the motor was adjusted until stereoscopic vision just appeared or disappeared. The results are shown in Table XXVII.

TABLE XXVII

Observer	Revolutions per minute	Interval between illuminations to produce stereoscopic effect
		sec.
A	160	·188
F	175	·171
G	160	·188

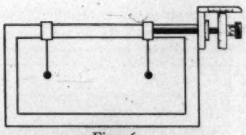

Fig. 16 *a*

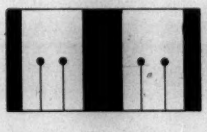

Fig. 16 *b*

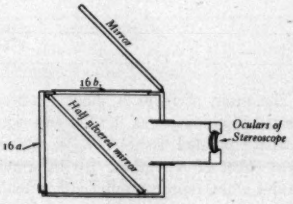

Fig. 16 *c*

The results are very similar to those obtained in Experiment 24.

In conclusion I must thank Professor G. B. Bryan, D.Sc., Head of the Department of Physics of the Royal Naval College, Greenwich, for giving me every facility and encouragement to carry out these researches. I must also thank various members of the classes of officers qualifying for (G) for their assistance, the laboratory attendant Mr White (Observer B) for his industry as an observer and

the mechanics of the department, especially Mr Collins, whose skill and experience in the construction of apparatus was of the greatest assistance to me in these experiments.

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DISCUSSION

Prof. L. C. Martin: There can be no doubt that Comdr. Langlands' paper is one of great importance, firstly because it brings to the notice of British readers a considerable amount of work which has not been well known hitherto, and secondly it explores the lines of enquiry and experimental methods by which our knowledge of binocular vision may be extended.

At the same time this preliminary work brings home the great number of variable factors as typified by the "education" factor which influence the results of such experiments. The directly antagonistic factor of "fatigue" is important, and

also the state of adaptation of the eye. While it is possible that some such factors are of less consequence in experiments on binocular vision than in experiments on colour or contrast perception, it would certainly be desirable to make some attempt at bringing them under control as far as possible. When this has been done it may be possible to obtain information as to the effect of varying a single factor, such as the brightness of the field, if the variation is taken over a sufficiently wide range. Thus the figures for Table VII in which the field illumination ranges from 4 to 50 foot-candles refer only to a range of brightness in which there is comparatively little variation of other visual functions such as contrast perception or monocular acuity. In addition, there is in Table VII the complicating factor of a variation in the angular subtense of the field, so that, as Comdr. Langlands himself observes, it is difficult to conclude anything definite from these results.

Furthermore there is always the possibility to face that two important conditions, *A* and *B* say, have antagonistic actions. In any given experiment the action of *A* may neutralise that of *B*, and no definite conclusion is possible until *A* and *B* have each been varied individually. Thus in the experiment (4) on extra-foveal illumination the conditions are really more complex than the simple contrast: extra-foveal illumination *v.* no extra-foveal illumination. It is quite possible that the external and internal angular radii of the outer stimulus field, and the brightness also, *may* have varying effects. I have met with specific examples of this character in my own work, so that the value of a single observation under arbitrary conditions is problematical.

In view of the great number of experiments which Comdr. Langlands has carried out, it may seem almost unkind to ask for more. On the other hand it may be considered that a series of trustworthy answers to all the problems which the paper raises would constitute a reasonably complete survey of the main phenomena of normal binocular vision, a field which is almost too large to be adequately explored by one observer. It is only to be hoped that these stimulating and suggestive experiments, such as on the relations between "form sense" and binocular acuity, will awaken the interest of students of physiological optics so that our knowledge of such things may be greatly extended. It is the best of news that Comdr. Langlands is now engaged in an extension of the work he has just described.

Mr E. F. Fincham: The results of Comdr. Langlands' measurements of the binocular sense under conditions of flash illumination are of great interest and value. The knowledge that such a wide variation exists in the binocular acuity of different individuals, and that in a given individual it is increased with experience, appears to be of considerable importance.

Some years ago a few simple experiments upon the binocular sense under conditions of illumination of short duration were carried out at the Northampton Institute. Fine needles were used as test objects, their extremities being excluded from the field of view. A roller-blind shutter, giving an exposure of 1/250 second, was placed before the eyes. Personally I found that, although one had no hesitation in saying which needle was nearer, when they were separated by 2 mm. at a distance

of 33 cm., the sense of real solidity was only obtained when the eyes were uncovered for a period of half a second or longer.

It would appear that, although the judgment of depth may be made by the interpretation of the differences in the two retinal images, the true stereoscopic sense is obtained only when these images persist long enough for the eyes to make a movement—probably the necessary adjustment of convergence in order to fuse consecutively the various points under observation.

Dr G. F. C. Searle: I have no remarks to make which bear specially on *binocular* vision, but it may be of some interest to mention the case of a new student who for several days had obtained absurd results on the optical bench. Eventually it was found that he would not attempt to decide on the position of an image until he had stared at it so long and so hard that he had paralysed his eyes. I then demanded that he should say whether a coin, which was exposed to his view for a fraction of a second, was of silver or of copper. The student was able to pass this test and was thereby convinced that only a short time was needed for making an observation. His difficulties in the location of images then passed away.

Other students, thinking that micrometer scales in eyepieces must be difficult to see, entirely fail to see them. The remedy employed is to distract the student's attention by telling him some mystifying yarn about a roof visible in the distance. He is instructed to look at this roof, keeping both eyes open, and then to bring one eye to the eyepiece. He then sees the scale and has no further difficulty.

Mr H. H. Emsley: The proof of Comdr. Langlands' paper has not been in my hands long enough to enable me to do more than glance at it. Obviously it contains some really valuable matter, the experiments having been carried out in a precise manner, and the results therefore forming a sound basis to work on.

I notice that the four observers have all got normal acuity. Has Comdr. Langlands tried any experiments with observers who are anisometropic; if so, what was the effect?

Did he notice the effects of fatigue and health during the experiments? In what manner were such effects dealt with?

Lastly, in those cases where an observer, in the flash experiments, made a false statement as to the position of the third object, was he allowed further attempts and was any difficulty experienced in these subsequent attempts? What I mean is this—in experiments I carried out some time ago on irregular refraction, a prominent feature was the "glimpsing" of the lines of the grating used, as described in my short paper* to this Society. An observer could clearly see the lines instantaneously and then lost them; often he could not resolve them again unless they were brought closer. This makes the recording of the results difficult.

Mr W. B. Coutts: Comdr. Langlands' paper is a comprehensive one and requires careful reading before it can be discussed fully. It should be of considerable value to those who are interested in stereoscopic rangefinders and other binocular instruments.

* *Trans. Opt. Soc.* **27** 1925–6 (28).

With reference to the experiments described in the first part of the paper—those on binocular acuity using constant illumination—I should like to ask Comdr. Langlands if these experiments showed any evidence of one type of moving mark giving consistently better results than others.

Mr O. Aves: As a refractionist I am very interested in Comdr. Langlands' paper. Binocular vision is a subject on which there appears to be necessity for a great amount of research work.

I should like to ask whether the various observers were tested for muscular imbalance in any other than the primary position and whether care was taken to ensure that the test objects were always placed in that relationship to the observers during the whole of the experiments. The reason I ask is that my own observations tend to show that muscular balance may be a very variable factor in the same individual if measured in other directions than the primary position.

Secondly, I should like to ask which particular stereoscopic slides of Prof. Pulfrich are referred to as being inadequate tests for depth perception. A further point is that it appears to me that the answers given for very short exposures must be largely a matter of guess-work rather than strictly accurate judgments.

The thanks of all who are interested in the subject of binocular vision are due to Comdr. Langlands and his colleagues for the very thorough series of investigations which his paper represents.

Comdr. Langlands (partly communicated): With regard to Prof. Martin's remarks it is certainly of the greatest importance to attempt to control the variable factors before reaching definite conclusions. Until we know something of the working of these factors it would appear to be of no great value to attempt to give an accurate numerical value for the binocular acuity of an individual or to make a statistical survey of the binocular acuities of a group of people. It also appears to the author that similar researches on "vernier acuity" are necessary before the advantages or disadvantages of stereoscopic against coincidence rangefinders can be discussed.

Mr Fincham's experiments at 33 cm. distance are most interesting since they can be at once roughly compared with Helmholtz's experiments at 34 cm. Binocular acuity and the true stereoscopic sense are probably as distinct things as are visual acuity and height and breadth form sense.

Dr Searle's method of teaching a student to see the micrometer scale by telling him to look with the other eye at a distant object brings to mind Köhler's method of fusing two stereograms by the unaided eyes by placing a piece of window glass inclined at 45° between the stereograms and the eyes and viewing the reflection of a distant object in the glass. On directing one's attention to the stereograms fusion is more easily affected.

With regard to Mr Emsley's remarks Greeff (a), p. 44) has discussed results obtained from observers who possess anisometropia. The effect of fatigue was noticeable in the author's experiments but it did not always appear at the same point in a series of observations. It can be checked by arranging the observations

in rows and observing the number of errors in each row. The effect of health has also been noticed in recent experiments. In the flash experiments the results of the first observation should be used. If subsequent flashes were observed, using the same position of the movable object, an oscillating effect often appeared and the middle object seemed to be alternately in front and behind the fixed objects.

In reply to Mr Coutts the type of mark was not tabulated in the experiments in which constant illumination was used and both the effect of the shape and size of the movable mark and the effect of its position in height and breadth with respect to the fixed marks require to be investigated.

With regard to the remarks of Mr Aves the muscular imbalance was tested approximately in the primary position, and during the observations the primary direction was nearly horizontal, the head being fixed.

The test plates of Prof. Pulfrich, which I have mentioned, are those of 1901 and 1907 and are referred to in the Bibliography under Pulfrich (3) and (4). The latter reference is to a small pamphlet, published by Zeiss in 1908, 7 pp., reference number, Mess 204.

CONTRIBUTIONS TO THE THEORY OF STEREOSCOPIC VISION

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MS. received, 5th November, 1926. Read and discussed, 13th January, 1927.

ABSTRACT. The present theories explaining the visual, vernier, and binocular acuities are critically discussed and an electron theory is put forward to explain these acuities.

Present Theories

In this paper we will first discuss the existing theories of the retinal mosaic in so far as these are directed towards explaining the magnitudes of the visual, vernier, and binocular acuities. The treatment of these is so closely allied that they will be dealt with in turn. The present theories will be explained. Such exceptions as have been made to them on experimental grounds will be examined. A modification of these theories will then be put forward.

Visual Acuity

First we must deal with visual acuity. We shall limit ourselves for the moment to the foveal region and consider only cones as recipient elements. The value of the diameters of the cones in this region varies from 1·5 to 2·0μ at the lowest to 4·5μ at the highest (Hofmann (1), p. 58). These diameters are presumably those of the inner ends (Innenglieder), i.e. the thick sections, of the cones (see Helmholtz (1), p. 33). Fritsch (1) showed that the diameter varied markedly in different persons from 1·5μ to 4 or 5μ. In the very beautiful microphotograph reproduced by kind permission of Professor Heine of the Augenklinik, Kiel, as Fig. 1 (see Heine (2)), the diameter of the cones is about 4μ. The cones form an approximately mathematical mosaic of hexagons for the most part arranged in almost straight rows. The retina is considered to be built up of contiguous hexagons. Each retinal element (cone) is assumed to denote one space value and one alone which is markedly different from that of its neighbours. If under any circumstances two points of light are just resolvable, their two retinal images must be so far separated that between the two illuminated areas there must be a less illuminated area. The images are formed so that they fall on two of the sections of cones which are separated by one unstimulated or more weakly stimulated hexagon (see Helmholtz (1), p. 256). The distance between the images, assuming for the moment an ideally sharp image which is never obtained in practice, cannot be smaller than the diameter of the retinal element. Hence the resolution of point sources of light

must depend on the angular diameter which the image of the section of the cone subtends in the object space. If the diameter of the retinal element be taken as 5μ, this means a resolving power of 60·5 seconds; if it be $1{\cdot}5\mu$, the resolving power would be about 18 seconds. Helmholtz quotes Hooke in stating that scarcely one out of a hundred can resolve two stars whose separation is less than 60 seconds.

In this treatment the existence of irregular image distributions due to anomalies of refraction has been neglected as have also been the so-called irradiation and the light distribution due to diffraction both along and at right angles to the cone directions. We can of course (after Mach) take the stimulated area of the retina and erect everywhere on it normals to its plane proportional in height to the retinal stimulation. We need then only consider that portion of the stimulated area whose stimulation reaches the threshold value.

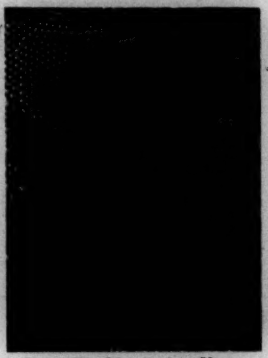

Fig. 1. The Mosaic of the Human Fovea. (Heine (2))

Vernier Acuity

We come to another form of acuity—the vernier acuity which was first measured by Wülfing in 1892. One portion of a plane surface is painted black and the other portion white, the two parts being separated by a vertical line. A horizontal separating line divides the surface into an upper and lower half. The half below this horizontal line is movable by a micrometer screw relative to the upper half. The screw is adjusted until the two vertical lines between the black and white areas appear to form one continuous line. Wülfing obtained values of 10 to 12 seconds for certainty of setting. Best obtained for such settings a 50 per cent. threshold of 2·5 seconds and a 100 per cent. threshold of 13 seconds. For a horizontal position he obtained larger values. When the lines were inclined he obtained values of 16–19 seconds for the 100 per cent. threshold. French (1), p. 129, etc.) found that the precision setting of vertical black lines on a white ground depended on the width of the lines and on their angular heights above and below the separating line. The precision error diminished up to a certain point when the angular height of the lines increased and then a further increase of the latter had practically no effect on the precision. With fine lines and suitable angular heights precision settings of under 4 seconds were obtained. From the consideration of such small numerical values for the vernier acuity it might be thought with Wülfing that the diameter of the retinal elements must be smaller than had been supposed or alternatively that one portion of an element must be able to transmit a different position in space compared with that transmitted by other portions of the same element.

In 1899 Hering(1) published a valuable paper in which he describes Wülfing's experiments. He considers that the fovea in section can be represented by a network of contiguous hexagonal elements. For double lines as for double points the distance between the retinal images must be greater than the diameter of a retinal element if the double line is to be resolved. Hering believes that the retinal image of a straight line in consequence of the nearly regular arrangement of the visual elements can only in quite special cases coincide with a straight row of elements but in general it stimulates a row of more or less zig-zag elements. In consequence of the perpetual small movements of the eye the image moves continually over the retina. Therefore the relative position in space of the elements stimulated by the image of the single line fluctuates about a mean position, which position is the determining factor of the perception.

Hering considers first the ideal case where the retinal elements are arranged in straight lines and he supposes that these are by chance parallel to the lines separating the black and white areas in Wülfing's apparatus. Fig. 2 *a* represents this special

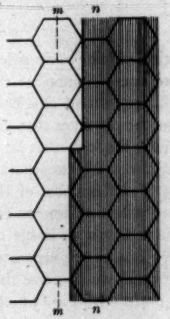

Fig. 2 *a*

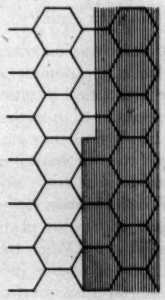
Fig. 2 *b*

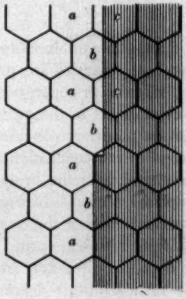

Fig. 2 *c*

case, and it is evident that the small shift shown in the upper half of the diagram would be sufficient to stimulate the elements in the row *nn* through the agency of the white part of the surface. As soon as the stimulation produced on this row is sufficient to become noticeable, the position difference between the two lines will be perceived. It is quite true, as is shown in Fig. 2 *b*, that a small movement of the retinal images will result in both half lines being brought on one and the same row of elements of generally equal width. But a small movement of the eye in the same or the opposite direction will once more move both half lines on to different rows of elements. Thus the difference in position at one instant vanishes and at the next instant reappears sufficiently markedly to ensure its perception. A second possible case is shown in Fig. 2 *c*, where the limiting lines between the black and white areas are parallel to the sides of the regular hexagons. The lower

half of the image of the limiting line runs alternately in the middle of one element *b* and along the line between the two elements. The lower half will be determined by the *a* and *b* elements, the upper half by the *c* and *b* elements and the upper half will be in a position perceptibly different from that of the lower half when the excitation of the *c* elements becomes sufficiently great. So far the cases described have been very special ones. To these must be added the case in which the limiting lines between the white and black areas have any inclination with regard to the rows of elements, and finally we must consider more or less divergent arrangements of the retinal elements themselves. But Hering reaches the conclusion that under favourable circumstances a shift of one half line over a fraction of an elementary diameter will lead to a perceptible change in the position provided that the retinal image of the boundary between the white and black areas falls at a small enough angle to the rows of cones. The value of the smallest perceptible change of position will depend on the sharpness of the retinal image, on the intensity of the illumination, and on the circumstances of the adaptation of the eye.

Binocular Acuity

In the case of binocular acuity we have a similar explanation. Heine (1) considers that both maculae are projected into one position so that one can imagine the two mosaics as superposed. The two optical images which are formed of a convenient object in the medial plane nearer to or farther from the observer than the fixation point are considered to be placed beside each other. He feels that he is forced to the conclusion that each cone or vertical row of cones has its separate transmission to the brain. "Thus there is really no limit to the fineness of the binocular acuity." In Fig. 3 *a*, *F* is the fixation element and a vertical line on the observer's side of the fixation point and in the median plane forms an image on the right retina at b_2 and on the left at b_1. When these two images are separated by $0{\cdot}5\mu$, i.e. lie $0{\cdot}25\mu$ from the point of fixation in each retina, we perceive that the vertical line is nearer the observer than the fixation point. In Fig. 3 *b* the vertical line is situated beyond the fixation point.

Summary

Such are the present theories explaining the visual, vernier, and binocular acuities. It will be seen that they depend eventually on the cone as the element of visual perception. We will omit the consideration of the function of the rods. (Nagel (1) has shown that in faint illumination the rods replace the cones also so far as stereoscopic vision goes.) These theories state that for true visual perception the cone must be directly stimulated by light and that different parts of the same cone are unable to transmit different directions in space. In the next section we shall show that various difficulties arise which are inadequately explained by the above theories and another line of treatment of the problem will be proposed which seems successful in elucidating these difficulties.

In the first place if we assume that the image formed by a point of light produces an image on the retina, whose area over which the threshold of perception is exceeded is less than the section of a cone, then the smallest movement of the eye makes this illuminated area stimulate two cones. Thus the point of light should appear doubled in area, which is well known not to be the case. The presence of

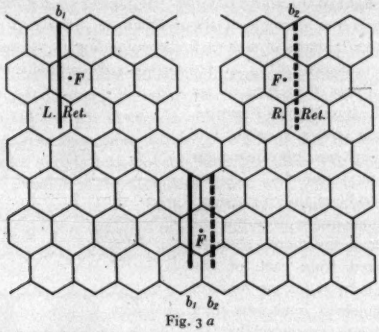

Fig. 3 *a*

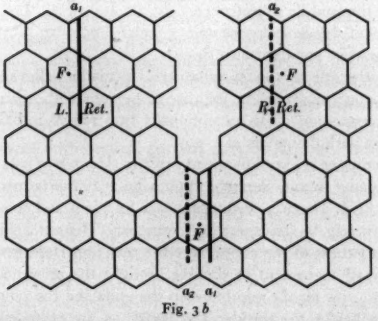

Fig. 3 *b*

physiological irradiation has been inferred from this, i.e. the stimulation of one element of the mosaic must be accompanied by the stimulation of neighbouring elements.

Hess has shown that the outer end structure (Aussenglieder), i.e. the thin ends of the cones and not the inner or thick ends (Innenglieder) are the light receiving elements. Consequently the diagrams in Figs. 2 *a*, 2 *b*, 2 *c*, 3 *a*, and 3 *b* are inaccurate. These should, as Hofmann ((1), p. 98) points out, represent sections through the thin ends and it would be better to represent the retinal elements by

a series of circles with gaps between them. Even if, however, we took a section through the thick or inner ends we should not get the arrangement of adjacent hexagons in contact given by Hering but an arrangement of hexagons with empty spaces between them.

Schoute (1) describes an experiment (see Hofmann (1), p. 61) which it is difficult to explain in the light of the above theories. He takes $4{\cdot}4\mu$ for the diameter of the cones, which represents an angle of about 1 minute. After experience he is able to perceive eight differences of size each corresponding to 7·5 seconds. The idea used by Schoute is shown in Fig. 4. It represents the retinal image of the object which he used. There is a ring of six objects each of which represents 1 minute, and each of which covers a cone. The angular distance between opposite images in the ring is thus 3 minutes. Schoute brings into the middle of the ring a small object whose retinal image does not exceed the diameter of the central element *a* and is separated from each of the images in the ring by at least 1 minute. He could distinguish at least four sizes of this object. Schoute considered that the retinal image of the middle object never exceeded the area of the central cone and that the differences in apparent size depended on the strength of the stimulation. Hofmann believed that, since the phenomenon was only observed in the case of persons with a very high acuity, it was due to the possession of an exceptionally fine retinal mosaic possessing say cones in the fovea with diameters of $1{\cdot}5\mu$.

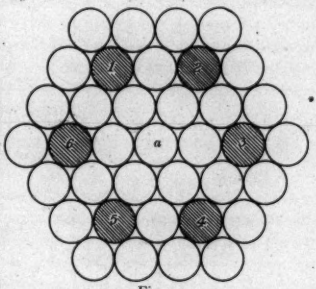

Fig. 4

Hensen's experiment is also difficult to explain. Hensen (1) uses either fine black points on white paper in a moderately illuminated room or faintly illuminated fine points on a dark field. These are reduced in size by a lens and observed by one eye. They seem alternately to disappear and reappear. Hensen held with Hess that the outer end structures of the cones were the recipient elements. Between these were gaps. A small movement of the eye brought the image now on the outer end of a cone and now on the gap between the ends and the points appeared and disappeared. We ought to consider the image as an extended distribution of illumination, the central portion of which may be above the threshold value for perception. This central area would be smaller than the gaps between the elements. The transition from appearance to disappearance is found experimentally to be gradual and not sudden. The phenomenon is not observed when both eyes are used. It has been explained as due to the natural light of the eye combined with movements of the eyeball.

In the case of vernier acuity it is important to make a careful examination of Heine's photograph of the foveal mosaic (Fig. 1). In this photograph the cones

are undoubtedly disposed in straight rows in many directions. Fixing on any particular cone and considering an area round it we can find a series of directions from this cone which have very few rows of cones in the adjacent area parallel in direction. We should not expect so fine a vernier acuity in such directions, but that, as the arrangement of Wülfing is rotated and measurements of the vernier acuity are made, periodic changes of value would be exhibited. Such changes have never been detected experimentally so far as I know. Best did not seem to have discovered them in the numerical values given earlier in this paper. He had actually advanced the theory that in simple diagrams containing parallel lines the acuity must be finest in three directions, each of which is at right angles to the opposite pairs of sides of the hexagons. But, as Heine(2) points out, neither his photograph nor experiments demonstrate the truth of this theory. Of course the position of the lines in these experiments is judged along the whole retinal image and not from the position of the stimulated cones where the retinal images of the two lines are nearest each other. Still, combining Hering's theory and Heine's photograph, we should have expected to find experimentally that for some directions the vernier acuity would be high, while for intermediate inclinations it would attain a lower value.

It is, however, when we come to consider the fineness of binocular acuity that these theories of the retina encounter the greatest difficulties. We saw in my earlier paper* how Helmholtz was led to consider erroneously that the visual acuity and the binocular acuity were similar in fineness. A whole succession of experiments demonstrated this error. In the subsequent theory of Hering and Heine we see that now the vernier acuity and the binocular acuity are associated. Hoefer(1) says that: "Recently the true limits of stereoscopic perception in its narrower sense were shown by E. Hering to be identical with the monocular visual acuity (for lines) of Wülfing." That may be the case if we confine our measurements of binocular acuity to experiments with vertical parallel rods or threads. In my earlier paper (Experiment 5) it was shown that for three points of light on a dark field a binocular 50 per cent. threshold of 5·0 seconds was obtained, which agreed almost exactly with the value which I had previously obtained, using Howard's apparatus with its two vertical rods. Such an acuity cannot be easily explained by Hering's theory. It might be considered that the fixation point could be altered until the images of the middle point of light fall on disparate cones on the two retinae. High acuity would therefore be dependent on the choice of the most suitable fixation point. But can this be done so as to detect such fine differences in binocular angle as 5 seconds? Besides using the electric flash and a fixation point we (Experiment 9) obtained finer binocular acuities than the angle subtended by the cone widths of even quite fine retinal mosaics. These, it is true, were the result of experience, but none of the observers in these experiments considered that this improvement in the acuity resulted from fixing any other point than the small fixation prism. We see therefore that these theories, especially so far as they apply to the binocular acuity, are by no means convincing when attacked on the grounds of experimental

* "Experiments on Binocular Vision," *Trans. Opt. Soc.* **28** 1926–7 (45).

results nor are they invulnerable when assailed by theoretical considerations. I remember pointing out these considerations to Professor von Rohr in Jena, emphasising the strange fact that Hering associated the binocular and vernier acuity after having clearly demonstrated the fallacy of associating the visual and binocular acuity. Professor von Rohr thereupon pointed out that Pulfrich and Hering in 1899 were chiefly interested in the application of stereoscopic methods to measuring instruments such as the stereoscopic rangefinder, etc., where objects consisting of vertical lines (in great part) come into consideration. I find, however, that Pulfrich (2) quite realised this point. He says that these fine visual acuities depend on special conditions such as are found in Wülfing's experiments, etc., but that the binocular acuity attains very fine values which are practically independent of the arrangement of the objects.

A Photo-electric Theory of the Retina

Let us follow Edridge-Green (1), chap. VIII) and Joly (1) in considering that the retina contains a photo-chemical liquid in which are placed a number of recipient elements, the outer or narrow end-structures of the cones. A section through the retina and these end-structures would show a series of approximately circular areas with gaps between them. The gaps are filled with the photo-chemical liquid. When light falls on a point in a gap the energy, presumably in an electron stream, flows out from this point and stimulates the neighbouring cones. Thus the cones are stimulated even although they are not directly acted upon by the light. Let us further assume that each cone is localised by the mind, not as in the existing theories as an area whose angular diameter varies between 18 and 60 seconds according to the observer's mosaic, but as a geometrical point on the retina or as associated with a direction line in the object space. In our schematic retina we can replace the cones by a series of points which we may consider as probably situated in the centres of the discs formed by the sections of the outer end-structures.

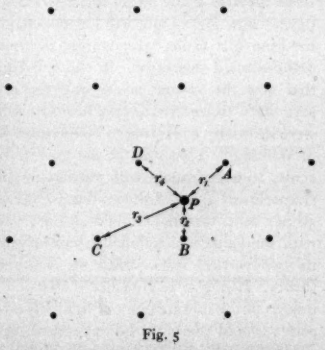

Fig. 5

Light is incident at P (Fig. 5) and energy travels out from P, reaching the neighbouring cones localised at A, B, C, D, etc. Let the distances PA, PB, PC, PD, etc., be r_1, r_2, r_3, r_4, etc. We might reasonably expect that the quantity of energy reaching a cone is a function $f(r)$ of the distance r from the point P to the cone and is independent of the direction of reception. Let us therefore assume that the quantities of energy reaching A, B, C, D, etc. are proportional to $f(r_1), f(r_2)$,

$f(r_3)$, $f(r_4)$, etc. We shall not hazard any hypothesis as to the form of $f(r)$ when expressed in terms of r. We can now consider that the point P is localised by the energy co-ordinates $f(r_1), f(r_2), f(r_3)$, etc., where there are of course certain relations derived from geometrical considerations between r_1, r_2, r_3, etc. It will at once be remarked that in such a case an increase in the amount of energy flowing from P will be accompanied by an increase in the accuracy of the localisation. If the localisation of P were independent of the retinal brightness within a given range of values of the latter, we could consider that the energies at B, C, D were compared with that received at A, i.e. the mind localised P by the co-ordinates

$$\frac{f(r_2)-f(r_1)}{f(r_1)}, \quad \frac{f(r_3)-f(r_1)}{f(r_1)}, \quad \frac{(r_4)-f(r_1)}{f(r_1)}, \text{ etc.}$$

or perhaps P could be localised by

$$\frac{f(r_1)}{\Sigma f(r)}, \quad \frac{f(r_2)}{\Sigma f(r)}, \quad \frac{f(r_3)}{\Sigma f(r)}, \text{ etc.}$$

where $\Sigma f(r)$ is the total energy. P of course would never be localised as an exact point. There would be an area inside which P would lie and inside which the mind could not detect any movement of P. The shape and extent of this area would depend on many considerations such as its position relative to the mosaic, the coarseness of the mosaic, experience, the effect of mental conditions on the judgment of the energy co-ordinates, etc.

Let us consider the visual acuity when two neighbouring points are stimulated in one retina. Two image points are formed and these both send out a stream of energy. They have common cones which receive energy from each point and the two energy distributions are superposed in the mind. Whether the ability to disentangle these two energy-distributions and so to resolve the two points of light is inherited or acquired by experience or both will not be considered here. When the two image points are sufficiently separated on the retina, the sources are resolved and the angle subtended by the two sources is the measure of the visual acuity.

Fig. 6 *a* represents the image of a bright line on the retina. Every point of the

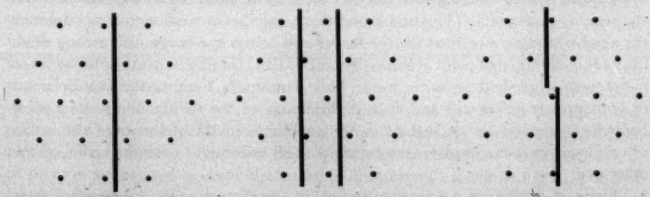

Fig. 6 *a* Fig. 6 *b* Fig. 6 *c*

image-line sends energy outwards and this stimulates the neighbouring cones. The line is localised by the energies which are transmitted along its whole length to neighbouring cones. The mind associates this energy distribution with a line.

Two parallel adjacent lines (Fig. 6 *b*) produce the sum of two such energy-

distributions and as in the case of points they can only be resolved when they are far enough apart for the two energy distributions to be disentangled by the mind. If the two lines are not side by side but (Fig. 6 *c*) one is shifted into the upper half of the retina and the other into the lower half, the two energy-distributions are practically distinct save for a few cones where the ends of the lines are adjacent. They are thus localised with much greater accuracy. Hence the vernier acuity is very much finer than the acuity for resolving two adjacent lines. As the lines are lengthened the localisation of each becomes more definite until a maximum accuracy of localisation is reached, as is shown by French (1).

The above theory is immediately applicable to the explanation of the fineness of the binocular acuity. Here one energy distribution is in one retina and the other in the other retina and both are distinct. For vertical rods and strings we have therefore a rather similar state of things in the case of the binocular acuity to what we have in the case of the vernier acuity when Wülfing's two lines are vertical. Both pairs of energy-distributions are distinct. It is interesting to note that Fischer (1, p. 213), cutting off the upper half of one retina and the lower half of the other, found that the horopter curve for white vertical threads against a dark background was very nearly the same curve as that obtained when the whole of both retinae were stimulated. The binocular acuity for points is, however, very different to the visual acuity for points. In the former we have distinct, in the latter superposed energy distributions.

We can define two corresponding points as two points, one in the left retina and the other in the right retina, which when stimulated produce energy-distributions in the neighbouring cones which are associated together as corresponding by the mind. Strictly speaking, we cannot have two similar corresponding points because an energy-distribution is really associated with a small area. We have, therefore, two corresponding areas in the two retinae.

The experiment of Schoute is easily explained in terms of the above theory. Hensen's experiment can be explained as follows. If we use one eye and faintly illuminated points, then one of these may form an image in one of the gaps between the recipient elements. The stimulation there may be so weak as not to stimulate the neighbouring cones until the eye moves and brings the image near a cone when the weakly illuminated point is gradually perceived. It is stated that the phenomenon is not seen when both eyes are used. This is probably because the fixation point of both eyes is never still and even if the image of the faintly illuminated point happens to remain for an instant in the gap between the elements of the retina of one eye, yet it is meantime executing a small movement over the retina of the other eye.

There is one point worth mentioning in connection with this theory. It is impossible to stimulate by light one cone without passing on energy to the surrounding cones (unless, of course, the light be very faint). Salzer (1) found three and a half million cones in the retina, and half a million nerve fibres which had to serve both the rods and the cones. We can follow von Fleischl, therefore, and consider that cones served by the same nerve fibre may be scattered some distance

apart throughout the retina. Since the normal stimulation of such a cone will stimulate the neighbouring cones there can be no doubt in the mind as to which cone is stimulated. For a very faint illumination doubt may exist and this may be in some way connected with the phenomenon discovered by Hensen.

The Stereoscopic Notion

In what follows we shall restrict the term binocular to the small portion of the common field of two eyes in which fine differences of depth can be detected. We shall follow Helmholtz (1), p. 609) in our use of the terms "perception," "idea," and "notion." "Perception" is applied to a sensation which is built up without any memory of an earlier experience. "Idea" (Vorstellung) is limited to the memory images of objects which are accompanied by no present sensation. A "notion" (Anschauung) is the combination of "ideas" and "perception."

The word "stereoscopic" will be restricted to the common field of both eyes. Depth perception or in its finest sense binocular depth perception is measured by binocular acuity.

Review of the Theories of Stereoscopic Vision

No one can now hold any rigorous theory of Identity, i.e. one to one correspondence between the elements of the two retinae. The empiristic theories of stereoscopic vision allow experience to play by far the largest part. The nativistic theories, of course, permit the consideration of the factor of experience but consider that the distribution of points seen in the common field leads to an innate conception of space while the depth perception is an act of judgment.

Panum first stated that the binocular parallax could give an immediate sense of depth relations. From his experimental knowledge he proposed a modification of the theory of identity in which one point a of one mosaic is identical with every point within a corresponding area A of the other and these points could fuse provided that a similar contour passes through the point in the area A to that which passes through the point a. At the same time different depth perceptions would arise when a was fused with different points in the area A. Panum knew that similar contours fused even when they were composed of non-corresponding or disparate points. Of course the nearer similar contours are to corresponding points, the easier they fuse. Dissimilar contours usually produce stereoscopic rivalry. The hypothesis, moreover, that when slightly dissimilar contours fuse one of them is suppressed is contradicted by Wheatstone's experiment that in such a case depth perception exists. Fischer (2) has recently used the "binocular vernier method" of Tschermak to investigate the size of "corresponding areas" of Panum (see Fischer (2), p. 239, Abb. 2). He found that the size of these areas decreased towards a limit with experience.

Helmholtz (1), p. 958) and Bergmann showed that corresponding lines both lying near two similar disparate contours whose similarity is not disturbed by the

presence of the lines hinder the fusion which is easily made when the corresponding lines are absent.

Hering supposed that the retinal points have three fundamental values—"height value," "breadth value," and "depth value." Ignoring the existence of the apparent verticals and referring these points to axes consisting of the true vertical and horizontal lines through the fixation point, the two former values are the co-ordinates of the points. The temporal retinal regions have positive and the nasal regions negative depth values. Corresponding points have thus as depth values quantities equal but opposite in sign.

Helmholtz ((1), p. 965) raises some weighty objections to Hering's theories. He thinks that experience triumphs over depth values, that the judgment of distance by means of double images depends on experience and not on depth values, and ((1), p. 968) that the Hering depth values vanish without a trace when the interpretation of the visual phenomena, based on experience or on the memory of the form of individual objects, contradicts them. Helmholtz ((1), p. 885 and again p. 957) emphasises the great importance of the experiment of Wheatstone ((3), p. 245) to be described now. In this experiment disparate images are fused in two stereoscopic pictures, whereas corresponding points are seen as double. Helmholtz's modification of this experiment is shown in Fig. 7. The lines *ab* and *cd* are disparate while the points *h* and *i* on opposite sides of the lines are corresponding. The fixation points are *f* and *g*. The areas *A* and *B* are coloured green (shown thus ▨), whilst *C* and *D* are coloured red (shown thus ▩). The two disparate lines *ab* and *cd* are fused when the two corresponding points *h* and *i* are seen in double image. If the attention be now turned from the general consideration of the two half pictures to the particular consideration of the points composing them, *ab* and *cd* are seen in double images whereas *h* and *i* are fused.

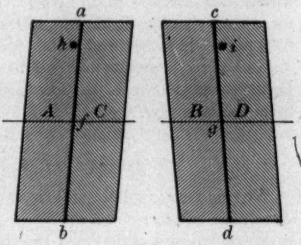

Fig. 7

Let us turn to the case of an observer using a stereoscope. His head is fixed so that no head movements can complicate the observations. In Dove's classical experiment it was seen that a stereoscopic effect appeared when the two half pictures were illuminated by an almost instantaneous electric spark. In my earlier paper under similar conditions of illumination a relatively fine binocular acuity was obtained. Heine in his most interesting paper ((3), p. 180) had made by a stereoscopic camera or its equivalent two pictures of three vertical rods of which two were in one plane and at the same distance from the camera and the middle rod was nearer the camera. These half pictures were mounted in a stereoscope and were each rotated in the same direction. As the angle increases up to the horizontal position for the rods the middle rod appears to approach the observer and to leave the two side rods. The plastic is clearly dependent on

the horizontal distance between points in the two images, i.e. on the horizontal disparity.

But we have another fundamental experiment of Wheatstone to consider (3), p. 260). When the convergence is altered by rotating one arm in his modified mirror stereoscope, the magnitude of the fused image appears to vary. When the convergence decreases, the image appears to increase in magnitude and vice versa. Heine ((4), p. 569, and (5), p. 309) has shown that for given stereoscopic half pictures the less the convergence the greater the plastic and vice versa. In other words this means that the use made of the difference in binocular parallax is different according to the estimation of the absolute distance of the object.

Let us turn to Tscherning ((1), p. 393). He seems to incline to Brücke's view of the importance of the ocular movements. He says *inter alia*: "Looking into a stereoscope, especially if the objects are difficult to fuse, it is only after I have permitted my look to wander for some time on the figures, fusing sometimes the images of the distant objects, sometimes those of the near objects, that relief appears to me." That is quite true. But the observer may be totalising the depth perceptions. Difference of convergence may be a factor in forming a complete stereoscopic notion, but the experiments of Wheatstone and Heine suggest that convergence is bound up with psychological considerations rather than muscular movements and their innervations. In the case of Dove's experiment I am not sure whether we are dealing with a stereoscopic notion or a stereoscopic perception. If we took two half images composed of dots which fused into an unknown geometrical design with a background of dots, I doubt whether a stereoscopic relief would result under instantaneous illumination. Probably such diagrams can only be seen in relief when the observer has fixed a good many points in the diagram and totalised the binocular perception, but I have not tried such an experiment. Of course difference in convergence may be a factor especially when the plastic is deep and the pictures are viewed under a considerable angle of convergence, but I doubt if it usually plays a direct rôle in the formation of the stereoscopic notion.

Wheatstone (3), p. 462) showed that two half pictures unequal in magnitude could be fused in the stereoscope. He stated the fundamental principle that "if the inequality of the pictures be greater than the difference which exists between the two projections of the same object when seen in the most oblique position of the eyes (i.e. both turned to the extreme right or to the extreme left) ordinarily employed, they do not coalesce."

Small degrees of anisometropia are compatible with perfect stereoscopic vision. The fixation points are the centres of the foveae and correspond in each retina, but all other image points are disparate, the disparity increasing the greater the angle between the object and the fixation point (see Erggelet (1)).

In the theory which I have outlined above in which the localisation is determined by the energy which reaches neighbouring cones it is seen that as we go away from the fovea and the mosaic becomes coarser the localisation would become less exact. This would be due in part to the increased distance between the elements but also

perhaps in part to the smaller use made of the extra-foveal region in the judgment of localisation. It is well known that the farther we are from the fovea the greater the disparity which can exist between two lines so that they may still fuse. This latter observation led Panum to assume that his sensation areas increased as he travelled outwards from the fovea towards the periphery of the retina and this is corroborated by the experimental work of Fischer ((2), p. 239). This fact undoubtedly assists in producing fusion in the case of anisometropia.

In the case of squint, according to Greeff ((1), p. 46), few or no cases are known where, after the operation has been performed and fusion in the stereoscope has been produced, positive results are obtained through measuring the binocular acuity by say Hering's apparatus. Stereoscopic fusion may be established but not binocular perception.

In the above brief summary the current theories of stereoscopic vision have been stated and a few of the more important observations have been described. From a consideration of these the following thoughts arise.

In observing an object which has only height and breadth and not depth there are two factors at work, the visual acuity and situated in a higher association centre the form sense. The patient of Goldstein and Gelb (1) had good central visual acuity but no form sense. So in vision with two eyes we have the binocular acuity which goes towards building up the binocular perception, and we have also the stereoscopic notion probably situated in a still higher association centre.

Take for example the case of the fusion of two similar but disparate contours. Two similar energy distributions are perceived and these being similar produce two similar notions which fuse into a stereoscopic notion. It is true of course that these similar contours direct the movement of the fixation point.

Now in Helmholtz and Bergmann's experiments where correspondingly situated lines lie near the disparate contours the fusion was hindered. The reason of this appears to be that when we concentrate our attention on these similarly situated lines the binocular perception which ensues comes into conflict with the stereoscopic notion. Two slightly unequal circles fuse because each presents a similar idea.

Take Wheatstone's classical experiment (Fig. 7). The two half pictures viewed generally present exactly similar ideas and result in a stereoscopic notion, but when we concentrate our attention on the details of the two half pictures we find that the binocular or stereoscopic perception is at feud with the stereoscopic notion and fusion ceases. The colours of the half pictures and the inconspicuousness of *h* and *i* assist the fusion of the monocular notions, i.e. the formation of the stereoscopic notion.

I have found an excellent illustration of this in Heine ((3), p. 192, and Taf. IX, Fig. 18). He took a stereoscopic photograph of a convex cylinder on which was drawn a chess board pattern of equal black and white squares. The photographs obtained were bounded by the upper and lower margins of the chess board pattern which appeared as curved lines concave towards each other. He placed the photographs in the stereoscope, the right photograph before the left eyepiece, the left before the right eyepiece, so that a pseudoscopic effect should be produced. The

result of the horizontal disparity should produce a concave effect but the two curves at the top and bottom of each photograph stifle the binocular perception. Most observers state that they see a convex cylinder, deriving the convex idea from the upper and lower margins. If the half pictures are cut down by a horizontal slit so that only the centre horizontal row of squares is visible, then the binocular perception triumphs and the observer sees a concave cylinder. In the case of two dissimilar contours the monocular notions are in conflict and one or other half picture or a patchwork of both is forced upon the attention, but the binocular perception may still operate and produce a depth effect. Since the binocular perception in its most exact form depends on a mental judgment of the energy co-ordinates, it can be fairly easily suppressed when it comes into conflict with the stereoscopic notion.

If two pictures are presented, one to each eye, and these are coloured differently, sometimes a mixture of the two colours is observed but more often a rivalry. Haldat and later Dove stated that fusion occurs when the amplitudes of the two vibrations are nearly equal or not very different. So little is known of the colour mechanism of the retina or the psychological interpretation of colour that a complete explanation of binocular colour mixing must be left over to the future. It seems, however, to the author that the initial sensation produced by colour may be of importance. In Pulfrich's (9), p. 37) recent pamphlet on stereoscopy and photometry from observations of the so-called stereo-photometric effect two colours are defined to produce equal retinal brightness when the lags between their retinal excitation and detection are equal. Is it not possible that since the eye is continually in motion the growth of the colour sensation may play some part in colour perception? Lehmann (see Pulfrich (9), p. 63) found that the best results were obtained from anaglyphs when the light passing through the two coloured filters was equal in intensity and complementary in colour. I am not aware that the stereo-photometer has yet been employed to show whether the binocular fusion of two colours occurs most readily when the colours have equal stereo-photometric retinal brightness, i.e. produce equal lags between reception and detection.

Finally we come to the case where two photographs, say of a landscape, are made from the same negative and are cut square or circular but mounted so that the margin seen in the stereoscope appears like a port behind which the landscape lies. We have an impression that the landscape has depth, yet no binocular or stereoscopic perception can exist. What we think we see is the result of the stereoscopic idea called into being by the landscape appearing to lie behind the port. The phenomenon is especially easy to see if the focal lengths of the eyepieces of the stereoscope are equal to the focal length of the camera lens employed. Then each monocular image is in its correct perspective. I have taken photographs however of the objects used in my experiments on binocular acuity and mounted these in this fashion, but I was never able to make myself believe that the movable object was in front of or behind the silhouette. There were no data whereby the stereoscopic idea could be created in this case; in the other the plastic of the landscape was easy to idealise.

It is indeed remarkable when one reviews the theories of so many scientists in the field of stereoscopic vision that so little of real value has been contributed since Wheatstone's great and classic papers of 1838 and 1852. It is sad to think that Wheatstone's work has never received due honour in England. The work of Brewster has been more frequently extolled. Yet can there be any comparison between the relative values of their contributions? One need only read their works in the light of our experimental knowledge of to-day and Wheatstone will stand out in his true perspective.

Finally I must thank Mr J. C. Dustan, R.N., for drawing the diagrams in this and in my earlier paper on "Experiments in Binocular Vision." I am under a debt of gratitude to Professor M. von Rohr, of Jena, a lover of Wheatstone, who first interested me in this subject and who, among many other acts of kindness, secured for me access to the Zeiss, University, and Augenklinik Libraries in Jena.

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DISCUSSION

Mr J. Guild: The author has given a valuable summary of previous theories of the retinal mosaic in so far as these relate to the acuity of the eye for determining small differences of direction in space of two simultaneously visible objects, and has added one more to the number of interesting speculations concerning the mechanism of reception by the sensitive elements of the eye. Unfortunately I have been unable, owing to pressure of time, to do more than just read through the paper and the following comments refer to points which strike me at first sight as difficulties which may or may not on careful consideration prove to be reconcilable with the essential feature of the author's theory. The diagrams of Figs. 5 and 6 do not merely represent the cones as points (which the author intends) but represent images of point sources of light as small compared with the inter-cone distances. When we realise that the image of a point source is a disc which in the best circumstances will fill up all the space between the nearest group of cones, even assuming the latter to be effectively located as points, the obvious simplicity of Fig. 5 disappears, and we have to conclude that the distances r_1, r_2, r_3, etc. which determine, on this theory, the direction locating power of the eye, must be the distances, not to the nearest and most strongly affected cones, but to a group of more remote cones. While the author has left the nature of $f(r)$, etc. out of consideration, there are two well known phenomena connected with acuity of certain types of optical setting, e.g., the familiar "diagonal cross-wire" type of setting, which appear to lead to opposite conclusions as to the nature of the dependence on r.

Except for very low field illuminations in which rod vision is playing an appreciably important part, the sensitivity of such settings is practically, if not entirely, independent of field brightness for all ordinary illuminations. This makes it clear

that the sensitivity does not depend on differences in the actual amounts of energy reaching the various cones but must depend on the ratios of these amounts, i.e., on some function of the ratios of the various r's.

The other phenomenon is that sensitivity for the settings referred to is very little affected by the thickness of the cross-wires. It is not completely independent, but the curve connecting sensitivity with thickness has a very flat maximum extending over a large range. Since the number of cones completely covered by the width of the lines may vary considerably without affecting sensitivity, the latter would appear to depend on differences rather than ratios of the distances to the cones effective in direction locating, since presumably it is the centre of the image from which such distances must be reckoned.

It may be possible to reconcile these conflicting conclusions by some amplification of the sketch of the theory which the author has given.

Another feature of the theory is that reception, i.e., the conversion of the radiant energy into photo-electric or photochemical energy, is presumed to take place in the space between the cones. While space precludes entering into a discussion of this point here, I feel that the evidence at present available as to the physical properties of the retina is overwhelmingly in favour of the view that the transformation of energy takes place within the body of the cones (or rods in twilight vision) and not in the spaces between.

Here, again, the objection may not prove fatal to the essential idea of Comdr. Langlands' theory, but at first sight it presents a difficulty.

Mr E. F. Fincham: Comdr. Langlands' outline of a new photo-electric theory of vision appears to me to be an ingenious attempt to explain the discrepancy between the fineness of vernier acuity and the visual resolution of separated dots or a grating.

If we assume, as is probably correct, that the apices of the foveal cones are the visual receptors, one wonders whether the author is justified in basing the disposition of these upon the arrangement of the hexagonal bases of the cones. As was pointed out in the paper, the mosaic of the cone bases in the foveal area forms a fairly regular pattern, the arrangement being maintained by the sieve-like limiting membrane through which the cones project. These cones, although only between 1·5 and 5μ in diameter, are, according to Greeff, 85μ in length. Their extremities have no very definite support, in fact nothing beyond the extremely fine processes of protoplasm which flow out from the cells of the pigment epithelium under the influence of light. It would therefore seem doubtful whether such slender bodies as the foveal cones would maintain a direction perpendicular to the surface of the retina; is it not quite possible that the extremities of the cones may be displaced with movements of the eye so that the regular arrangement shown by Comdr. Langlands would no longer obtain?

I have wondered whether such a displacement, if it did exist, would account for the occasional flashes of remarkably high acuity which are found to occur, at least in some meridians, when using a grating test. This phenomenon, which was men-

tioned by Mr Emsley in his paper on "Irregular Astigmatism of the Eye"* might possibly be due to the momentary adjustment of the cone extremities so as to bring them into a more favourable arrangement for the resolution of lines in the particular meridian under test.

Dr R. S. Clay: On an occasion when a somewhat similar problem was discussed some years ago I suggested that it is probable that one is able to locate the position of a bright or dark line on the retina to a higher order of accuracy than the diameter of a single cone would have afforded, as a slight movement of the line as a whole would cause it to overlap new cones, and by long experience we are able to interpret such variations of stimulus. Our sub-conscious intelligence is marvellously clever in analysing such stimuli. I had occasion recently to note this in some experiments which Sir Richard Paget and I have been carrying out on the differences in resonance which distinguish the consonants from one another, so that I have no difficulty in believing that a similar analysis is possible in the case of the retina.

Mr T. Smith: Comdr. Langlands has pointed out that observation with the eye gives two magnitudes for the acuity which differ decidedly in value. It may be as well to point out that the larger of these, represented by the resolving power for point sources, is hardly an eye property at all. Distinction is impossible on account of the nature of light itself, from which it results that there is no sensible drop in the brightness between the image centres. The smaller magnitude, which is obtained from a variety of experiments, and includes the binocular and vernier acuities, on the other hand appears to represent a real property of the eye. The retinal images with which this smaller magnitude is concerned, unlike those of the former class, are distinct from one another. The real problem now is to reconcile this smaller magnitude with a retinal structure having a spacing corresponding to the larger magnitude.

It seems to me that many theories can be suggested, but a decision calls for knowledge which we do not at present possess. Is the refractive index of the cones relative to that of the fluid in which they are immersed known, so that we may know whether light may be expected to be refracted out of the cones or reflected to the tip? Is it known whether the cones are normally in motion or whether they are capable of motion but can be controlled? Whatever answers are given to these questions a suitable theory, consistent with the quantum character of light, can be offered to account for the accuracy in observation which these experiments reveal. It is worthy of note that time is an essential factor in the process, both because many quanta must be received to form a recognisable image and because time on a coarser scale is needed to reach a judgment with which the mind is satisfied when high accuracy is attempted. It may also be remarked that a conscious effort, which is distinctly tiring, is needed, and this suggests mental control of the eye in a way that is not usually called for. Are movements within the eyeball involved in this constraint, or is it merely the muscles which control the eye as a whole which are concerned? I find that however much I try I cannot keep my attention restricted

* H. H. Emsley, *Trans. Opt. Soc.* **27** 1925–6 (28).

to an area as small as that corresponding to resolution. Is this wandering a relevant factor? Perhaps experiments with flash illumination would help to give an answer. These questions seem relevant to such an enquiry as that on which the author has embarked, and I hope that some at least of these facts may soon be known.

Mr M. Blood: I might mention two small points. First, with regard to resolution at different angles—when resolving fine rulings, etc. under a microscope, I have tried rotating the stage, but have never been sure of better resolution at any particular angle, when the illumination was strictly axial.

Secondly, as an example of the accuracy of "vernier acuity" with ill-defined objects I have tested a magnifying rifle sight, in the form of a long low-power Galilean telescope, the front lens at the muzzle carrying a black ring as a definer, through which the bull's eye is seen. Obviously the ring or the bull's eye must be out of focus, yet I found a maximum error of alignment of only 10 secs. of angle.

I have tried another definer consisting of two black spots of the same apparent size as the bull's eye, giving the impression of three "bulls" in a row, but of course not all in focus. Though I have not tested this quantitatively, I found it in practice *too* sensitive, as such a small error of aim is noticeable, so that it is difficult to get a satisfactory shot away, the subconscious attempt at an impossible accuracy often leading to worse results.

Even more curious is the fact that in a mist or rain, when it is almost impossible even to see the bull's eye, one sometimes makes a string of "bulls." Though the "bull" is some 3 minutes in diameter (1½ minutes maximum error) the group of gun and cartridges under perfect conditions is usually at least 2 minutes in diameter. On a recent occasion I finished a score at 1100 yards with 9 "bulls," when conditions were so bad that I was not *certain* that I was aiming at the target at all, for any single shot! Moreover this was done with a poor batch of ammunition, so that the margin of error could have only been a small fraction of 1 minute. Personally I cannot understand or explain this, but it happens from time to time.

Comdr. Langlands: In reply to Mr Guild, the diagrams in Figs. 5 and 6 are, of course, only ideal. At the present stage it was hardly thought justifiable to have elaborated the theory further. Mr Guild criticises the theory on geometrical and physical grounds, but the theory is not a geometrical one. He appears to have overlooked its psychological aspect.

It is simply stated that recipient elements near a photo-electric disturbance receive a certain number of quanta of energy $f(r_1)$, $f(r_2)$, $f(r_3)$, etc. from the disturbance. From these quantities or from functions involving these quantities, which may be called "energy coordinates," a "notion" is formed in the mind. For example, a point source of light forms a volume diffraction image on the retina at P. The mind associates the energy coordinates, not with a diffraction image on the retina at P, but with a point source.

In his discussion of the two apparently contradictory phenomena of optical setting, Mr Guild in one case considers that functions of the ratios of the r's must be taken into account, in the other case he introduces differences of the r's. But,

according to the theory, the simplest quantities which can be considered are the $f(r)$'s.

If the first phenomenon be considered, namely that the sensitivity of the cross setting is approximately constant over a considerable range of brightness, it must be remembered that $f(r)$ involves two variables. First, it must contain a position variable r which will give the position of the recipient element relative to the disturbance. Secondly, it must contain a quantity Q which must depend on the number of quanta incident on the retina within a certain action radius. The first phenomenon will be explained, when we have chosen for the coordinates which fix the image such functions of the $f(r, Q)$'s as will be independent of the incident quanta Q over a given range of values of Q. The second phenomenon is that sensitivity of setting and thickness of cross-wires are approximately independent over a wide range. Surely the equality of the vernier acuities for areas and lines shows that a thick line is located by its edges and not by its centre, which is determined by a sort of mental bisection. If this be so, there is no difficulty in selecting the energy coordinates so that they will both satisfy the condition imposed by the first phenomenon, and in the second phenomenon locate the edges of the image by recipient cones lying just outside it.

So far as can be seen, there is no conclusive evidence at present that a photoelectric effect does not take place between the cones. The work of König, Kühne, and their successors on the absorption of retinal purple in solution is not against such a theory. Nagel sums up the position by remarking that it is doubtful how far present knowledge applies in the case of the eye of a warm blooded animal with circulation intact.

Mr Fincham's remarks are most interesting, as are his microphotographs. A prepared, stained, and cut section may, however, be in some ways misleading. If the recipient elements are easily displaced, glimpses of high acuity might be explained. It must always be remembered that such glimpses may be due to psychophysiological processes.

THE THEORY OF THE LENS-TESTING INTERFEROMETER. PART I

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ABSTRACT. An interferometer is in the best adjustment when the optical images of the same part of the incident beam formed by the two reunited beams are coincident. This implies in general a difference in the optical lengths of the interfering beams from source to image. Attention is directed to the importance, in the use of the camera lens interferometer, of focussing the photographic plate or the eye on the surface of the spherical mirror.

INTRODUCTION

TWYMAN'S adaptation of the Michelson interferometer for figuring and testing lenses, prisms, and other optical apparatus is well enough known to need no illustration. The principles governing its use in the examination of lenses over an extended field have been stated by Twyman*, and a method of converting its indications into terms with which lens designers are more familiar has been described by Perry†. The indications given by the interferometer have been found in good agreement with those of computed rays by Perry, and in agreement with the paths of rays revealed in the Hartmann test by Butkow‡, who adopted Perry's routine. Kingslake§, using a simple prism interferometer with a fine pinhole at the focus of a well-corrected lens as source, and dispensing with a collecting lens, obtained an interferogram by placing a photographic plate in the path of the approximately parallel united beams. This interferogram was measured and an analysis of the aberrations present made in a manner analogous to that employed in the analysis of Hartmann plates. Discrepancies between the amounts of the aberrations derived from the interferometric and Hartmann tests were found greatly exceeding the probable errors of either method.

The principles of the interferometer are essentially so simple, and the fundamental properties of light are applied so directly in the Hilger instrument to determine the defects of the system under test that, with a proper procedure, and with

* F. Twyman, "An Interferometer for Testing Camera Lenses," *Trans. Opt. Soc.* **22** 1920–1 (174).

† J. W. Perry, "The Determination of Aberrations, as expressed in Geometrical Optics, from the Indications of the Hilger Interferometer," *Trans. Opt. Soc.* **25** 1923–4 (97).

‡ Butkow, "Über die experimentelle Bestimmung der Aberrationen mit Hilfe des Twymanschen Interferometers," *Z. f. Physik*, **30** 1924 (268–276).

§ R. Kingslake, "The Analysis of an Interferogram," *Trans. Opt. Soc.* **28** 1926–7 (1).

all the essential parts of the apparatus in their correct relations to one another, the accuracy of the resulting measurements of the aberrations in terms of wave-lengths can hardly be questioned. When the determination of the greatest outstanding errors only is required, or when the conversion of the errors into another system of variables is performed, as in Perry's method, by making known displacements of the mirror, no difficulties arise, and the agreement found with quantitative results obtained by other proved methods would be confidently anticipated. At times it will be desired to record the performance of optical systems photographically, and the conditions which simplify the analysis to which such interferograms may be subjected are of interest.

Since there are two beams of light derived from the same original disturbance it is clear that fringes will be present in any plane intersecting these beams. In the absence of aberrations the fringes will present the same features in all normal sections of the beam, but when aberrations are present the fringes will change continuously in character as the plane of section is moved towards or away from the lens under test. The distribution of the light depends only on the properties of the lens and the relative positions of the optical parts of the apparatus, and if these are known it is theoretically possible to deduce the magnitude of the various aberrations for any known position of the photographic plate. Whether all positions are equally convenient is another matter.

In the physics of the interferometric test there is no ambiguity, but in methods of test based upon geometrical theories of image formation, as in the geometrical method of analysing aberrations, an arbitrary quantity is introduced. This arises from the custom of specifying rays by the points in which they intersect a stop rather than of using line coordinates. In tests such as Hartmann's, and in calculations, this additional quantity is given a definite value initially by selecting a surface arbitrarily (usually a plane surface) as the locus of points which identify the several rays proceeding to the image from a given object point. At first sight it would appear that in analysing interferometric records the choice of a reference surface may be equally arbitrary, since the aim is merely to associate variations in path with particular parts of the lens aperture. Actually, when precise measurements are required (and it is only under such circumstances that an analysis of an interferogram is undertaken), the choice to be preferred is quite definite.

The Projection of Fringes

The fringes recorded in a photograph are of course those existing where the photographic plate was placed. Similarly the fringes seen by an observer are those formed on the retina of his eye. Since fringes will often require to be recorded for positions in which it is physically impossible to place a plate, an optical system must frequently be used to secure a record. Any optical system projects fringes in precisely the same way as it forms images of more material objects. Let us suppose that two waves of the same frequency travelling towards a lens—not necessarily along the same paths—are in the same phase at a point P of the object space. We

are to regard the two waves as independent. Each displaced point of the ether serves as a source from which new disturbances proceed. Since the time taken by the light to travel from P to its image p is the same for the various paths through the lens, the identity of phase at P will be reproduced at p. Similarly, if one wave is half a period behind the other at Q, this precise difference will be repeated at q, the image of Q. If therefore the photograph is taken with the aid of an auxiliary optical system, the fringes are to be interpreted as belonging to the surface conjugate to the plate.

It should be particularly noted that we can infer from the plate nothing about the fringes in any other plane but that to which the plate is conjugate. We can imagine, for instance, several different pairs of beams which will give the same interference pattern in a given plane but differ elsewhere. It follows that the photograph will only be a complete record of the performance of the instrument under test if the camera is carefully focussed upon the surface where the differences of phase give a direct measurement of the errors.

The Correct Focus in a Lens Interferometer

The lens interferometer is essentially an instrument for comparing a real lens with an artificial optical system free from aberrations. The light beam from the artificial instrument is that reflected at the plane mirror, but the perfection of the artificial instrument is represented by the perfect sphericity of the spherical mirror at which the other beam of light is reflected. The errors of the real instrument are represented by the variation in phase of the light from point to point of this spherical mirror at the moment of reflection. The surface of this mirror then is the place where the variations of phase are significant, and it follows that the photographic plate should be focussed to secure a sharp image of this surface.

It may be observed that the whole interferometer is essentially an elaborate instrument for the observation of Newton's rings, and the theory of these rings is directly applicable to the fringes seen by the aid of the interferometer.

The Effect of Errors of Focus

If in either photographic or visual use of the instrument a different surface is sharply focussed, say the front glass surface of the system, an error will be made in the estimate of the faults in the system, and this error will be the more serious the greater the magnitude of these faults. The error arises from the fact that the path variation is attributed entirely to the lens zone in which the returning light meets the lens surface, when in reality the outgoing light, which on refraction contributes to the observed variation, has passed through a different zone. The error recorded is therefore the mean of the error due to the observed zone and that of an unknown zone. When the interferometer is used to correct local defects this source of inaccuracy is gradually removed as the work proceeds, and finally inaccurate focussing becomes of no significance as perfection of form is attained.

Evidently the uncertainty due to inaccuracy of focus can be diminished by using a mirror of the largest permissible radius, so that the outgoing and returning beams pass through zones which are near to one another.

Correction to Perry's Theory

The theory developed by Perry involves the stop magnification, and as the theory is given the value to be attached to it is uncertain. Apparently he inclines to assign it the value for the lens stop itself. From the considerations which have been advanced above it is clear that this is incorrect and that a value should be taken depending on the ratio of the radius of the mirror to the focal length of the lens when the latter is focussed for an object at infinity.

The Effective Aperture Stop

It will be seen that the aperture surfaces upon which the fringes are recorded are in general inclined at an angle other than a right angle to the axis of the lens. If the analysis is to be based upon a fixed stop in the object space, the measurements on the plate in conjunction with the shadow of the lens aperture will form the basis of the conversion. If a stop in the image space is chosen it will be convenient to engrave a reference system of concentric circles and straight lines upon the spherical mirror, which will be recorded in the photograph. Incidentally the circles will enable any departures from the generalised sine condition to be measured. It should be noted that a reference system of this kind is of greater physical utility than those customarily used in geometrical optics, and it is something more than an accident that in an instrument based on the physical character of light it is the more significant system which forces itself on our attention.

Visual Focussing Lens

When the instrument is used visually the pupil of the eye is placed near an image of the centre of curvature of the mirror in order to receive light simultaneously from the whole aperture of the lens. Since the surface of the convex mirror is necessarily nearer the observer than the centre of curvature, the eye is called upon to focus on the retina light converging to an image behind the eye. It therefore seems desirable to introduce a negative lens in the neighbourhood of the image of the centre of curvature of the mirror to produce a virtual image of the mirror surface in front of the observer.

Tolerances

In the preceding paragraphs the use of the interferometer has been discussed on the tacit assumption that the whole instrument is free from defects and that the light admitted is incident on the mirrors only in the direction corresponding to the centre of curvature of the spherical mirror. In any real instrument the parts can at most have defects less than a given finite value and the incident light will

fall in various directions corresponding to the size of the opening in the collimator disc. It will now be shown that the presence of small defects and the finite solid angle occupied by the incident light may be neglected in the use of the instrument.

The Mirrors

It is quite clear that defects in any of the three mirrors employed in the instrument produce errors of phase of approximately equal amounts. The errors in all these parts should therefore be reduced to a fraction of the errors considered significant in the lenses to be tested, so that no combination of errors in these auxiliary parts will produce a significant modification in the interference pattern recorded in any test.

The correct relative adjustment of the mirrors, so that the half-reflecting mirror bisects the angle between the plane mirror and the quasi-plane mirror consisting of the combination of the lens under test and the spherical mirror, is accomplished by the observer. Slight inaccuracies of adjustment displace the fringes, but do not introduce errors into the calculated aberration provided the theory to be applied allows for the presence of these errors. In discussing other errors we may therefore neglect errors in the shape of the mirrors and maladjustment in this respect.

The Cone of Light

It has already been pointed out that the aberrations of the system are theoretically deducible from an interferogram no matter what position the plate occupies. This possibility, however, when the plate is not focussed upon the spherical mirror, depends upon a number of factors of which one is precise knowledge of one of the interfering beams. It will suffice if one of the beams travels in a single known direction, i.e. if it is accurately collimated and free from aberration. These conditions were in fact found necessary by Kingslake in his experimental investigation. Their non-fulfilment would involve more detailed analysis of the problem.

When the plate is focussed on the surface of the spherical mirror very considerable latitude in these respects is permissible. In consequence of the finite size of the entrance hole of the collimator, waves will leave the collimator lens somewhat inclined to the principal axis, and the behaviour of the interferometer proper and also of the lens and spherical mirror combination under this more general illumination have to be considered.

It will be shown in the first place that, in the absence of aberration, the lens and spherical mirror are equivalent to a fixed plane mirror for light incident approximately normally on the spherical mirror. In Fig. 1, AA' represents a plane wave front of light advancing to meet the first surface LL' of the lens under test, by which it is refracted as a spherical wave front converging to O the centre of curvature of the mirror RR'. If the rays AL and $A'L'$ are produced beyond LL' to rr', and Lr, $L'r'$ are made equal to the optical lengths LR and $L'R'$, the reflected beam will behave as though it had encountered a plane mirror placed

normally to the beam at rr'. This is not however the image of RR'. The image will in general be oblique to the incident beam and will be curved; let it be IJI'. Through a convenient mean point J on this image draw the plane PJP' normally to the beam. If I and I' are the conjugates of R and R', the path between I and R, for rays which would pass near O if not interrupted by the mirror, is independent of the ray along which it is measured; similarly the path between I' and R' is stationary; but these two stationary values are different. The difference is obviously equal to that between rI and $r'I'$ or between PI and $P'I'$. For both the outgoing and the reflected beam we therefore have to associate with each several point I, J, I' of the mirror image a constant path correction if we wish to replace the lens and spherical mirror by its equivalent in the initial medium. In considering the direction in which a reflected beam proceeds it is only necessary to consider the differences in the corrections from point to point, and not their absolute values. Imagine then an incident

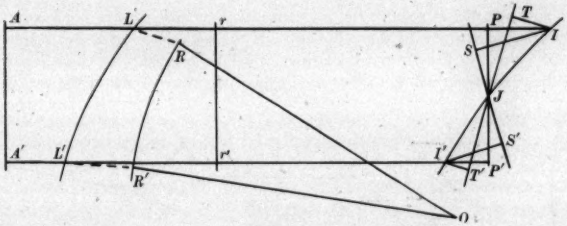


Fig. 1

plane wave SS' passing through J. Let IS and $I'S'$ be perpendiculars on the wave from I and I'. The reflected wave front TJT' corresponding to the incident wave front SJS' will be the envelope of spheres centred at I, I' with radii $2PI - SI$, $2P'I' - S'I'$, where I and I' are regarded as typical points of the surface. If the inclination of SS' to PP' is not large it is obvious that TT' will be a plane equally, but oppositely, inclined to PP'. In other words the lens and spherical mirror are equivalent to a plane mirror PP' for light falling over a finite angular range, with the reservation that the actual path length to the spherical mirror and back is less than that to the imaginary plane mirror PP' by the constant distance $2.rP$.

If the lens is not free from aberration the equivalent mirror PP' will depart from the plane to precisely the extent that the incident wave front fails to make general contact with RR' at the same instant. The errors of the lens are therefore quite correctly represented by equal errors in the mirror placed at PP', and it only remains to show that when properly adjusted the interferometer will record correctly the errors of a mirror at PP'.

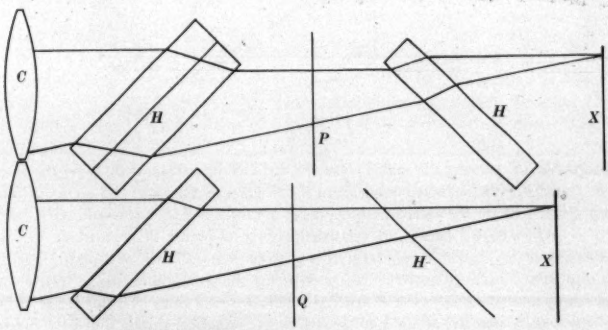
In a compensated interferometer with perfect mirrors it is very easy to see that obliquity in the light is without effect. If one of the mirrors is imperfect, the interferometer will measure its defects, whatever the obliquity of the light, provided

the recording surface is focussed upon the defective mirror. The accuracy of this record when all the incident waves are plane, though not all incident from the same direction, is unaffected by an inequality in the total path lengths of the two beams; it is merely necessary to keep the difference within the range outside which satisfactory interference fringes cannot be obtained.

When the interferometer is not compensated, the arguments used earlier can be applied directly to show that intervening media may be omitted by replacing a mirror by its optical image. It follows that the interferometer can be employed satisfactorily with a collimator hole of finite size, and without any compensation, provided the component waves may be regarded as plane and the recording surface is focussed on the spherical mirror.

Collimator Corrections

It now remains to remove the last restrictions, which involve good correction and precise adjustment of the collimator. If the collimator gives out a spherical wave or a wave not free from irregularities, it will be apparent that these departures from the plane form will not affect the image if the wave fronts of the two beams



C = collimator objective
H = half-silvered mirror
P = plane mirror met by reflected beam
Q = plane mirror or the equivalent met by transmitted beam
X = a section of the reunited beams

Fig. 2

entering the observing telescope are identical in shape and position when they severally establish first contact with the telescope lens. In other words, if the images of the collimator lens formed by the two beams are superposed in the united beams, any irregularities in the incident beam are rendered harmless. It is interesting to trace in detail the way in which this compensation is effected. A single example

will suffice—the neutralisation of the lateral displacement of a ray in its unsymmetrical and uncompensated passage through the half-reflecting mirror. With a rectified representation of reflection the two paths corresponding to a single incident ray may be represented as in Fig. 2.

The secondary downward displacement of the ray shown in the upper part of the figure, due to the inexact balance of the inclined ray at the two additional glass plates, encountered with opposed slopes, is compensated by the additional real length of this path. The addition arises from the fact that a length t in the glass counts only as t/μ towards the corrected length, where μ is the refractive index. The optical paths, where t counts as μt, will differ by a still greater amount than the real paths, but this difference is common to all parts of the field.

The Telescope

It is evident that the telescope is merely required to give good definition. It is a convenience rather than a part of the measuring appartaus, and its properties need no special consideration.

Conclusion

The conclusions to which this investigation has led may be derived from the principle that the adjustments of an interferometer should be made by paying attention to correspondence in optical imagery rather than to equality in the physical paths of the interfering beams. The latter only becomes significant when adjustment for image correspondence involves path differences so great that interference cannot be obtained.

Neglect of this image correspondence necessitates more severe restrictions in the light admitted to the instrument and in other respects.

This principle is applicable to other types of interferometer.

DISCUSSION

Mr R. Kingslake: Mr Smith suggests that the convex mirror is the surface at which we require to measure phase differences. This is certainly convenient in many respects, chiefly because each "ray" through the test lens strikes the convex mirror at one and only one point, whereas, owing to the aberrations of this lens, any ray will enter and emerge through it at slightly different points. Moreover, by this choice of reference surface, all difficulty as to the correct plane for the cross-wires is removed, because, as Mr Smith suggests, they could be marked on the surface of the convex mirror. There would be some difficulty in centering these marks relative to the optical axis of the lens under test, but this should be surmountable by sufficient care in the adjustment of the apparatus. The graphical process which I have used to transfer the pattern from the photograph back to the

lens itself is laborious and rather uncertain, but this projection method makes all further "correction" of the pattern unnecessary.

The objection to the use of the convex mirror as reference surface is its arbitrariness. In testing a given lens, we may use any mirror of any actual radius (within the limit of the focal length of the test lens), and thus we can measure the aberrational path-differences of the lens at any arbitrary plane. Some method would have to be devised, therefore, to convert these aberrations to, say, the exit pupil of the lens, in order to give them an absolute significance independent of the radius of the mirror used in the experiments. If any projection lens were used, it would be essential to mark latitude and longitude lines on the convex mirror, and to refer the coordinates of the interference bands to these lines. If this were correctly done, the aberrations of the projection system, which of course includes the test lens, would introduce no errors. It is to be noted that we should be trying to focus a convex surface on a flat plate, but no doubt depth of focus would cover this.

As regards the aberrations of the collimator, I have frequently observed a curious apparently zonal defect in telescope objectives tested with the simple collimator, which disappears when an achromatic collimating lens is substituted. Mr Smith's remark that it is t/μ which is important instead of $t\mu$ is interesting, but it would appear to be very difficult to secure this desirable relation between the "lengths" of the two interfering paths. In practice one moves the sliding plane mirror until the sharpest pattern is seen, but this does not ensure the kind of equality required; and I should like to ask Mr Smith if there is any simple routine method of equalising the paths in this desirable way, to render the collimator aberrations ineffective.

I must thank Mr Smith for this excellent summary of the properties of the lens interferometer, and particularly for the non-mathematical presentation. The theory of the interferometer lends itself to non-mathematical treatment by the very simplicity of its construction, but there are many aspects of the instrument which require deeper mathematical treatment, and no doubt Mr Smith will elaborate these in a further paper.

Mr J. Guild: I think much of the difficulty which has arisen regarding the interpretation of interference effects observed with the camera lens interferometer is due to the fact that the development of this instrument was preceded by that of the prism interferometer. The type of work done on the latter instrument is of such a character that considerations such as those dealt with by Mr Smith may be safely neglected, and in the simple technique which it is usual to apply to such instruments they are, in fact, neglected, with the result that their existence tends to be overlooked. Users of the camera lens interferometer have tended to assume that the loose technique of the prism interferometer may be applied to the testing of lenses, where the departures from an ideal wave-front are relatively enormous compared with those encountered in testing prisms or flats. In the latter case, even with only moderately good work, the departures from the ideal wave form are so small that the light ray traverses the article under test at sensibly the same point of the

aperture on both its journeys. In fact the actual separation in space of a ray before and after reflection is negligible at any reasonable distance from the back mirror, and the fringe system will appear the same in whatever plane it is viewed. In the case of a prism or plate showing defects of the same magnitude as a camera lens at oblique incidence, it would be necessary, just as in the case of the lens, to observe the fringes in the plane of the back reflector if a correct interpretation were required. In practice, however, with a prism or flat no one would want to "interpret" such fringes, but would either proceed to remove them by refiguring, or else scrap the article.

What has to be borne in mind is that with the optical system used in these interferometers the fringe system exists at all distances from the observer between plus and minus infinity, and may be seen or photographed in any plane. This paper shows that in the presence of considerable deformations in the wave-front the fringes must be observed in a certain locality, namely, at the back reflector, in order that the appearance may be most easily interpreted in terms of the aberrational properties of the lens under test.

Mr Smith: The reference of aberrations in geometrical optics to a plane through the exit pupil is a conventional procedure which happens to be simple. As in so many optical problems a surface of which the actual shape required to fit the theory adopted has not been investigated has been assumed to be a plane. The use of a spherical surface centered about the image point is in many respects preferable. In any event for first order aberrations a comparatively crude conversion formula is good enough. It may be possible to discuss the reference surfaces on another occasion when a mathematical theory of the interferometer has been given.

The method to be used in adjusting the plane mirror is a matter for experiment. It should not be difficult; for example a simple focussing device viewing an object placed over the collimator aperture should enable the adjustment to be made with ample accuracy. The errors due to inexact focussing, it will be observed, have not yet been considered.

REVIEWS

Commission Internationale de l'Éclairage: Recueil des Travaux et Compte Rendu des Séances, Sixième Session, Genève, Juillet, 1924. Pp. 432; 95 Figs. (Cambridge: University Press, 1926.) Price 15*s.* net.

The fields of work adopted by the Commission are wide; the contents of this volume, of which nearly half are in English, show that they include among others the investigation of scientific problems fundamental to photometry and photometric standards; the work of the illuminating engineer; the social and legal aspects of proper lighting whether of schools, factories, shops, public halls, homes, roads, or vehicles; and propaganda having for its aim the fuller realisation by the public of the importance of good illumination. The greater part of the volume is occupied by papers dealing with progress in the adoption of good lighting in various countries and kindred subjects. The scientific papers in the volume deal with such subjects as the properties of tungsten and methods of realising a primary standard of light of the black-body type; they all appear to have been published in the same or in a more detailed form elsewhere.

One very important section of the proceedings remains which is of particular interest to the physicist—that dealing with photometric definitions and symbols. The reports published in the volume indicate that the various national bodies carrying out this work are in substantial agreement with one another. International agreement on this subject and on practical units is indeed important. That agreement along these particular lines should apparently be achieved with such facility is surprising, and even somewhat disquieting; for if agreement can be beneficial it must be presumed that agreement along some other lines can be detrimental, and in our view the line followed by the Commission and by the several constituent national committees is not scientifically sound.

The fundamental problem with which the Commission is concerned is the measurement of the illumination obtainable from certain sources of light and the rating of these sources in terms of recognised units. Of the methods known to science by means of which such measurements can be made, we need only consider the direct basic method in which light is received by the eye. The facts to be taken into account which enable a rational scale of illumination to be constructed are well known, and are the foundation upon which the laws of photometry are built. The impossibility of employing the eye as a measuring instrument except to give the negative verdict of no difference in sensation, and the wide distinction between the growth of sensation and the increase of illumination by which it is caused, are among these vital facts. For instance, it is almost axiomatic that with illuminants differing in colour a statement about the relative illuminating power is meaningless unless additional information is given—e.g. an adequate statement about the conditions and methods adopted in obtaining a comparison.

Instead of bearing these facts in mind when constructing their definitions, the Committees have virtually constructed a theory of illumination and made the definitions, which should be dependent only on experimentally established facts, depend upon the adoption of their special theory. Moreover the theory put forward is unknown to physics. Its fundamental conception, that of luminous flux, has no physical reality. The edifice built upon this foundation is extensive, elaborate, and misleading. The definitions proposed are far more numerous than the subject requires, or rather than would be needed in a system acceptable to the physicist. One of their most serious defects is that the difficulties of the subject tend to be concealed by the adoption of a carefully chosen form of words; in this

respect they are the very antithesis of what the scientific worker desires or is likely to adopt. Definitions should promote clear thinking, not prevent it.

As an illustration the definition of luminous flux itself may be taken. "Luminous flux," so the official English rendering runs, "is the rate of passage of radiant energy evaluated by reference to the luminous sensation produced by it." We may ignore the obvious comment that the definition is meaningless by reason of the vagueness of the terms used, and the further objection that the radiant energy has ceased to exist before any sensation can have been aroused by it. If it means anything the definition implies a scale of sensations, but it is applied subsequently for the numerical evaluation of the stimulating radiation. Apart from its other faults the system is therefore self-contradictory. The position is not materially improved if we disregard the actual wording and attempt to substitute the unexpressed intention. It is a fundamental defect of the system that it does not at the outset recognise that light and sensation are alike in comprising diverse parts which cannot be expressed in a common measure. It is not of course to be supposed that the authors of these definitions were not aware of these facts, but they have clearly failed to recognise the penalty paid by tacitly ignoring them. As so often happens when ideas are expressed in terms not exactly fitting the truth, errors are committed because the line between the true and the false is wrongly placed, and this displacement is forgotten. Have all the Committees for instance realised the implications of their resolutions as they affect heterochromatic photometric standardisation? Illustrations of errors from similar causes might be taken from other branches of optics. With these there was at least the excuse that they were believed to be true when they were formulated. Such a plea cannot be accepted in mitigation of the faults of this photometric system. The only really satisfactory course would be to cancel the entire set of definitions and make a fresh start on a basis consistent with the known relevant facts.

Wireless Pictures and Television. By T. THORNE BAKER, F.Inst.P. Pp. x + 188; 100 Figs. (London: Constable and Co., Ltd., 1926.) Price 6*s.* 6*d.* net.

Now that wireless telegraphy and telephony have become part and parcel of our daily life, considerable attention is being directed in the newspapers to what is sometimes described as the next wonder of science, namely television. Many claims to success have been put forward by various inventors or their newspaper interviewers, and it is rather difficult to determine from the available descriptions what progress has really been made in the last few years. The present book, which has been written by one who has himself worked for many years on the subject, fills a much-felt want. The greater part of the book is taken up with a description of the various systems of transmitting pictures to a distance, the last chapter being devoted to a discussion of the special problems connected with television. The many technical difficulties that must first be overcome before complete success has been attained are dealt with in a straightforward manner. The author is to be congratulated on his success in compressing a great deal of useful information into small compass. The book is well-provided with illustrations of apparatus, wiring and other diagrams, and examples of the results which have already been obtained with the various transmitting systems. There are very few mistakes, the main one noticed being that Fig. 33 on p. 66 has been printed upside down. The book is intended to be a semi-popular introduction to the subject and as such can be highly recommended.

Penrose's Annual: The Process Year Book and Review of the Graphic Arts, vol. XXIX. Edited by WM GAMBLE, F.R.P.S., F.O.S. Pp. xvi + 127. (London: Percy Lund, Humphries and Co., Ltd., 1927.) Price 8*s.* net.

The present volume contains the following articles: "A Review of Process Work," "The Work of the Private Presses—Essex House Press," "Pantone," "Multi-colour

Proofing," "The Isotonic Notation," "The Stuart Menzies Style of Advertising," "Shippers' Tickets," "Mechanical Typesetting Machines," "The Measurement of Tone Values," "The Munsell Colour System and the Need for a Standardisation of Colours," "Vehicles," "Steps towards Standardising Collodion," "Colour and Monochrome in Photography," "Methods of Testing Colours for Permanency," "The Use of Paper for Process Negatives," "Screen Distance and Stop Aperture," "Looking Backward," "Correcting Converging Perpendiculars in Architectural Subjects," "The Camera in Lithography," "An Advance on Etching Methods," "The Saint Bride Foundation Technical Library," "Duotones and Offset Printing," "Letterpress Printing *versus* Rotary-gravure," "On the Uses of Collotype," "Taste, Craftsmanship and Machinery in the Printing Industry," "Via Media," "Multi-colour Reel-fed Offset Machines," "The Heliochromic Process," "Superseding the Leather Roller in Lithography," and "The Editor's Note Book." The volume contains numerous illustrations representative of process work during the year 1926, in addition to plates illustrating the articles.

ABSTRACTS OF PATENT SPECIFICATIONS

254530. St Clair, A. R. COLOUR FILTERS.

A screen to simulate daylight consists of a translucent blue layer of a tint of 1·0 to 5·0 on Lovibond's tintometer, together with a red or yellow tint between 0·05 and 0·30, or both. The filter is used in conjunction with frosted glass.

254649. Vogel, J. M., and another. COLOUR CHARTS.

A colour chart system, arranged according to tints, shades, and greys, enables every colour to be determined and defined numerically. The primary colours and their mixtures with each other and with white are classified, so that any colour can be specified.

254739*. Haeseler, J. A. PROJECTION APPARATUS.

Consists of a portable apparatus for the stationary projection of films. The film is cooled by a current of air from an electric fan, and is fed step by step by fingers engaging perforations in the film, and reciprocated by an electromagnet.

255023*. Westinghouse Brake &c. Co. SIGNAL LAMPS.

The lamp includes two bulbs, working in conjunction with coloured screens, providing two coloured beams, which are preferably projected along the same path, one by reflection at a glass surface, the other by transmission through it.

255027*. Westinghouse Electric &c. Co. TELEVISION APPARATUS.

The image to be transmitted is projected through a mosaic colour screen on to a photo-sensitive surface which is searched by a cathode ray, thus controlling the modulation of transmitted oscillations. A similar synchronised apparatus is located at the receiving end.

255128. Vickers, Ltd., and others. TELESCOPES.

An erecting sighting-telescope giving a magnified image includes three simple or achromatic lenses in addition to the eyepiece lenses, arranged with their successive focal points in coincidence. Observation is made through a small aperture at the anterior focal point of the front lens of the system.

R. J. T.

* Specification published before acceptance.